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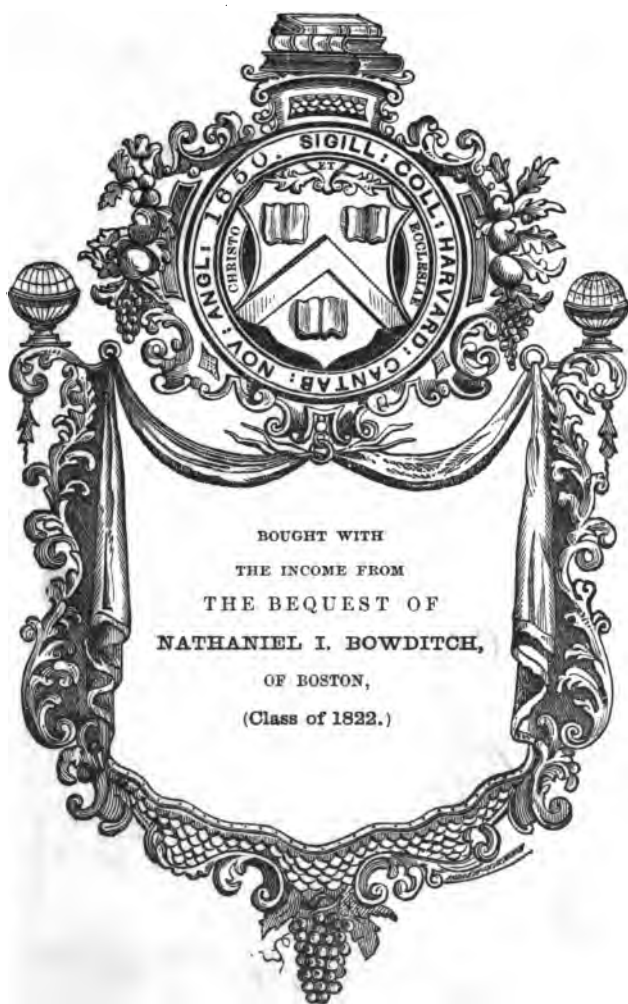
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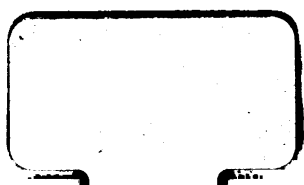
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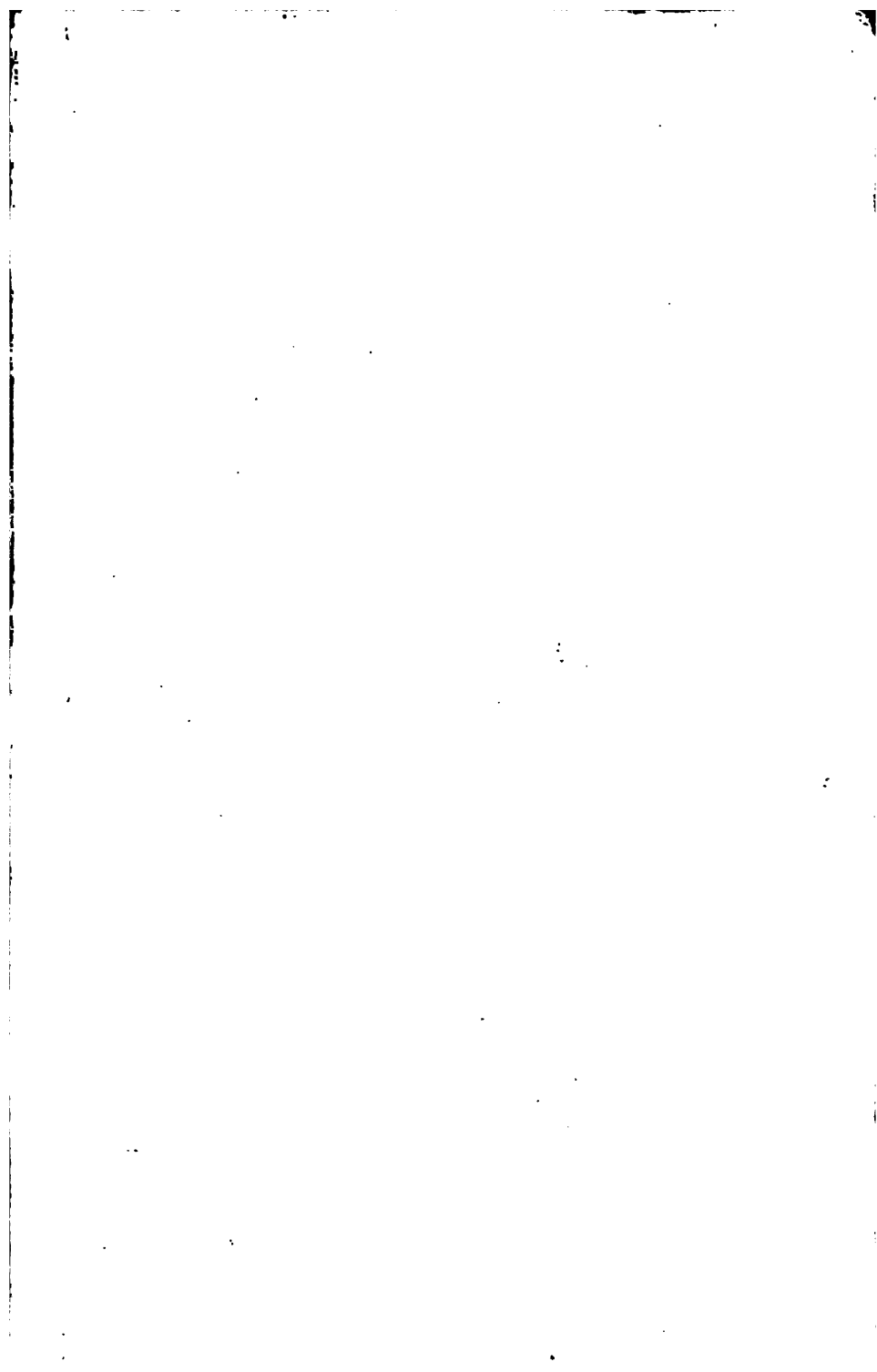
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THE REV. RICHARD SHEEPSHANKS, M.A.
F.R.S. F.R.A.S. &c.

Yours very truly
R. Sheepshanks

A Letter

TO THE

BOARD OF VISITORS

OF THE

GREENWICH ROYAL OBSERVATORY

IN REPLY TO

THE CALUMNIES OF MR. BABBAGE AT THEIR
MEETING IN JUNE 1853, AND
IN HIS BOOK ENTITLED *THE EXPOSITION OF 1851*.

BY THE

REV. R. SHEEPSHANKS, M.A.

ONE OF THE VISITORS.

(With Correspondence prefixed.)

Lædere gaudes,
Inquis; et hoc studio pravus facis. Undè petitum
Hoc in me jacies? est auctor quis denique eorum
Vixi cum quibus?

LONDON:

PRINTED BY G. BARCLAY, CASTLE ST. LEICESTER SQ.

1860.

Astr 293.4

✓ *



Boorditch fund

*" Chi potendo vietar, non vieta il male,
È partigian della ribalderia;
E chiunque è gentiluom naturale,
È obbligato per cavalleria
D'esser nemico d'ogni disleale,
E far vendetta d'ogni villania;
E se qualch' un di voi questo disprezza,
In se non ha bontà nè gentilezza."*

BERNI, *Orlando Innamorato*, lib. i. canto xxi. stan. 6.

MICROFILMED
AT HARVARD

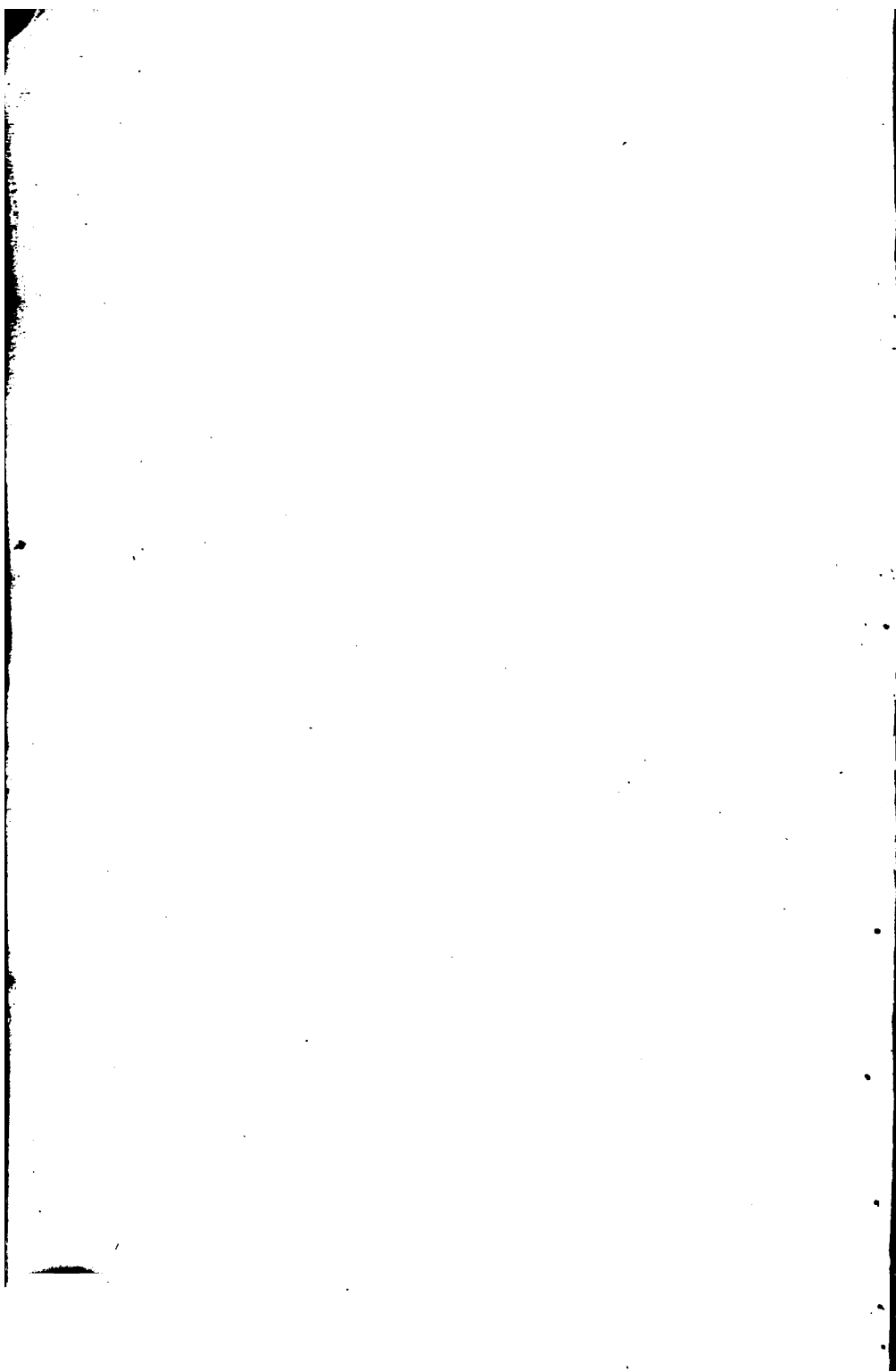
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ADVERTISEMENT.

A FEW copies of the reply made by the late Rev. Richard Sheepshanks to charges preferred by Sir James South and Mr. Charles Babbage having been found among the effects of the deceased, it has been thought advisable to present them to various public libraries, &c. with certain explanatory additions. Some separate copies of the additions will also be distributed.

The additions consist entirely of reprints, excepting only three very brief notes in brackets [], explanatory of allusions to facts. The matters reprinted are :—

1. Letters written by Sir James South and the Rev. Richard Sheepshanks to the Editor of *The Times* newspaper, or to each other, in 1838, and published in that journal.
2. Extract from Chapter XII., headed 'Intrigues of Science,' of "*The Exposition of 1851; or Views of the Industry, the Science, and the Government, of England.* By Charles Babbage, Esq. . . ." (2d edit. London, 1851, 8vo. pp. 154–172.)



*Letters written by Sir JAMES SOUTH and the
REV. RICHARD SHEEPHANKS to the EDITOR
of "The Times" Newspaper, or to each other,
in 1838, and published in that Journal.*

I. September 29, 1838. — *Extract of a Letter from Sir JAMES
SOUTH to the EDITOR.*

The kind commiseration of my friend Professor Encke for the wants of British astronomers having furnished them with an ephemeris of his comet, computed by himself, from the 1st of August last, it was easy to determine, even in July, where in the heavens to direct a telescope with a tolerable certainty of finding it, provided it was visible.

Accordingly, as in the search for Halley's comet, thinking it a public duty to shake off as far as possible my antipathy to enter my large observatory, where, for the gratification, I regret to say, of ecclesiastical malevolence, has stood for seven long years without having produced a single astronomical observation worth recording, one of the finest telescopes in existence, I looked for it from midnight to incipient daylight on the 13th, 15th, 19th, 24th, and 29th.

* * * *

Fearing lest the mounting of the telescope, which, when out of the meridian, makes apparently every star a planet, and every nebula a comet, should lead me to an erroneous inference as to the nature of the nebulous I had found, and thereby render me a public laughing-stock, I adopted the tedious, the almost barbarian, possibly the antediluvian, though oftentimes, for want of adjacent stars, the impossible mode of settling the question, by mapping on

paper the nebuloid's place relatively to some fixed stars, which most fortunately, in this instance, happened to be in the field with it.

* * * *

P.S.—As the planet and comet-making property of this instrument, spoken of in the eighth paragraph of this letter, may appear too ludicrous to obtain credence by those who have not had ocular demonstration of it, I would just state, that the instrument being moved through four hours of right ascension (which is about the time I observed the comet on the night of its discovery), will give a fictitious motion to any celestial body in its field, amounting to 11 minutes of a degree, or a quantity greater than one-third of the sun's diameter!

II. October 16, 1838.—*Extract of a Letter from Sir JAMES SOUTH to the EDITOR.*

Ere, however, I return you my best thanks for the ready admission into *The Times* journal of what I have from time to time transmitted you, there is one duty to be performed, and the Berlin recollections of this day render it most appropriate.

Friends of all ranks, lay as well as ecclesiastical, have called to my notice a sentiment contained in the fourth paragraph of my letter in *The Times* of the 29th ult., of which the following is a copy:—

“Accordingly, as in the search for Halley's comet, thinking it a public duty to shake off as far as possible my antipathy to enter my large observatory, where, for the gratification, I regret to say, of ecclesiastical malevolence, has stood for seven long years without having produced a single astronomical observation worth recording, one of the finest telescopes in existence, I looked for it from midnight to incipient daylight on the 13th, 15th, 19th, 24th, and 29th.”

To those who know me I need not say, the term “ecclesiastical malevolence” escaped me unintentionally, and implies a censure which my soul abhors. Brought up in the good old Tory principles which separate not “Church and King,” I have lived in them, and will most assuredly die in them. Amongst so large a flock as the clergy of this country, who are men like ourselves,

there must be some bad sheep ; but, as a body, I most unfeignedly respect them; I believe them to be true to their God—loyal to their Queen—and the greatest blessing to us all. Instead, then, of “ecclesiastical malevolence,” let me write, as I ought to have written, “of the malevolence of the Rev. Richard Sheepshanks, of Trinity College, Cambridge.”

III. November 5, 1838.—*Letter from the Rev. RICHARD SHEEPSHANKS to Sir JAMES SOUTH.*

30 Woburn Place, Nov. 1, 1838.

Sir,—On my return to England, I find that I have been the object of your especial notice in *The Times* newspaper. I regret that so long an interval should have elapsed between the attack and the reply, but I saw your letters yesterday for the first time.

On the 29th of September you say, “Thinking it a public duty to shake off as far as possible my antipathy to enter my large observatory, where for the gratification, I regret to say, of ecclesiastical malevolence, has stood for seven long years, without having produced a single astronomical observation worth recording, one of the finest telescopes in existence, I looked for it from midnight till incipient daylight on the 13th, 15th, 19th, 24th, and 29th;” and on the 16th of October, after a repetition of this passage, and an appropriate profession of orthodoxy in church and state, you add, by way of explanation, “Instead, then, of ‘ecclesiastical malevolence,’ let me write, as I ought to have written, ‘of the malevolence of the Rev. Richard Sheepshanks, of Trinity College, Cambridge.’”

“Name his name, and tell them plainly he is Snug, the joiner,” was the solution proposed by the magnanimous Bottom on an equally weighty emergency; and I am glad that you have at last dropped all mystery, and followed the honest sign-painter, who wrote “This is a white horse,” or “This is a blue lion,” for the edification of passers-by, to whom his brush had conveyed no such image.

And now to reply to your revised and deliberate assertion. I allow the fineness of the telescope, and also that for seven long years no observation of yours worth recording has proceeded from your large observatory; but I affirm, that on two or three fine

nights, when Messrs. Airy, Simms, and myself were authorized to enter it, a few very good measures of double stars, *in distance*, were taken with this much-abused equatorial. These distances agree precisely with those of Bessel, and in my opinion, almost decide the doubt between his measures and those of Struve, in favour of Bessel. Your statement, therefore, though true as regards yourself, is inaccurate with respect to others. If the sentence is further intended to imply that your long inactivity has not been perfectly voluntary, so far as I am concerned, I must give it a flat and positive contradiction, and I hereby challenge you to the proof.

The accusation of malevolence is an imputation of feelings which *may* be known to the supposed ill-willer, but *must* be collected by every one else from a consideration of facts. These you have not produced, and I need not tell others that to prove a negative is impossible. So long as the lawsuit between Messrs. Troughton and Simms and yourself is pending there would be a manifest impropriety in entering into the merits of the case. In the meantime, I must satisfy myself with the declaration that there is not a jot of foundation for your charge; that I have no control over your observatory, your equatorial, or yourself (unless I were a dealer in the black art, how could I?); that I have no gratification in your indolence, or incompetence, or insane antipathies; nay, that the very sentence which contains this inculcation does, in substance contradict it; for, when your sense of public duty had awakened you from your long and unquiet doze, you could and did "look for an object from midnight to incipient daylight," on every night you pleased, and you also made observations which you have thought worth recording.

It is two years since I saw your large equatorial, and "the mounting of the telescope, when out of the meridian, did" *not* then "make any star a planet, nor any nebula a comet." I speak, as to the past, from my own knowledge, and I do not believe that it does so now; for how could so faint an object as Encke's comet have been "very well seen" at two hours and a half from the meridian, with a telescope which effects such extraordinary metamorphoses? Again, I assert that, unless the equatorial be grievously deteriorated by neglect, "the instrument being moved through four hours of right ascension does" *not* "give a fictitious motion to any celestial body in its field, amounting to 11 minutes of a degree," *nor to a tenth part of that quantity*: I speak as any

astronomer would understand me, after allowing for the errors of adjustment, and determining the place of an object by the declination circle. The ordinary error was somewhere between 30 and 40 seconds of space, as I have the means of showing to those who can understand the nature of the proof. Thirdly, if you adopted "the tedious, the almost barbarian, possibly the antediluvian, though oftentimes, for want of adjacent stars, the impossible mode of settling the question" (i. e. of the comet's motion), "by mapping on paper the nebuloid's place relatively to some fixed stars," you only showed what I have always said—that your resources, as a practical astronomer, are of the most limited kind, and consist merely in manual and optical dexterity. I always suspected that you were quite abroad when out of Troughton's sailing-chart, and I thank you for this confirmation of my opinion. Lastly, I congratulate you on the discovery, that you were able to sweep for, find, identify, and ascertain the place of Encke's comet, with this much-abused equatorial. These involuntary admissions are not without importance, and their value is enhanced by the obvious want of skill in the observer.

I have the honour to be, &c. &c.

R. SHEEPSHANKS,

Fellow of Trinity College, Cambridge.

To Sir JAMES SOUTH, *Observatory, Kensington.*

N.B.—Persons who do not know either Sir J. South or myself, and who are not practically acquainted with astronomical instruments, may think my assertions scarcely more trustworthy than his; I therefore propose the unerring test of experiment, backed with the bait of a wager. I engage, with Sir J. South's permission, to put his large equatorial in working order (if it be not wholly degraded by neglect), at my own expense; and if, after it has been exhibited by me to any indifferent and competent observers—for instance, Sir J. Herschel, Captain W. H. Smyth, R.N., Rev. W. R. Dawes, of Ormskirk, &c., they, or any of them, shall be of opinion that I have exaggerated one tittle, I undertake to beg Sir J. South's pardon, and own my mistake in his large observatory, and further

to submit to any lawful punishment or reasonable fine that Sir James shall propose. If I am found to have spoken nothing but the truth, my "malevolence, ecclesiastical" or civil, will demand no further "gratification."

IV. November 15, 1838.—*Extract of a Letter from Sir JAMES SOUTH to the EDITOR.*

SIR,—Since my communication to you of November 3 was forwarded on November 4, I have seen that there has been published in *The Times* something or other signed by the Rev. Richard Sheepshanks, at the head of which I perceived my name. At the moment I send this, acting on the old adage, that "the lion in pursuit of the panther turns not aside at the braying of the ass," I know the contents of his reverence's communication only by report. If, however, some of them be such as I hear them represented, and as circumstances not under my control may prevent me for some days replying to his reported impertinences, let me state thus publicly, that well-authenticated documents, deposed to on oath, by persons of unblemished character, are in existence, which indicate in terms too plain to be misunderstood that in matters concerning me and my observatory that rev. gentleman's word must be received with caution.

In the mean time, let me transmit you documents of more public interest, which I received some days ago, but which circumstances, into which I need not enter, advised me to withhold a little while.

V. November 18, 1838.—*Statement of the Rev. RICHARD SHEEPSHANKS, in reply to Sir JAMES SOUTH.*

"At the moment I write this, acting on the old adage that 'the lion in pursuit of the panther turns not aside at the braying of an

ass,* I know his reverence's communication only by report."—Sir James South, *The Times* newspaper, Nov. 15.

Lion.—Then know that I, one Snug the joiner, am
A lion fell, nor else no lion's dam.

* * * * *

Theseus.—A very gentle beast, and of a good conscience.

Demet.—The very best at a beast, my Lord, that e'er I saw.

Lysand.—This lion is a very fox for his valour.

Theseus.—True; and a goose for his discretion.

When a disputant takes to his vulgar tongue, standers by may have a good guess at the state of the argument. In answer to my civil letter of the 5th instant, Sir James tells the readers of *The Times* that he is a lion and I am an ass, which, though lively and modest as an incidental remark, is no reply. I do not quarrel with my peaceable likeness. An ass once spoke briefly and to the purpose, which is more than is reported of the lion or his two-legged similitude; but the slighting use of the word "reverence" does a little surprise me in so stanch a Churchman as Sir James South, and is not very pertinent to Encke's comet.* Sir James and I are not at issue in matters of theology or pharmacy, and if we were why mix up our respective professions in the dispute? My skin is too tough to blister under his cantharides; but, should my endurance fail, I will not scandalize the Worshipful Company of Apothecaries in abusing him. Sir James further says, that he has in reserve undoubted testimony against my astronomical knowledge and against my credibility. *De non apparentibus et de non existentibus eadem est ratio* is an old and sound adage, though whether it is within Sir James South's scholarship or comprehension, I dare not be positive. Let the documents be published, and I undertake, "thus publicly," to prove them erroneous if they contradict me astronomically,—false if they impeach my veracity. I have offered the ordeal of actual experiment, which is worth all the testimonies and affidavits of half the

* That Sir James South is the lion, and that I am the ass, is "evident to any formal capacity;" but who is the panther? M. Boguslawski or Lieutenant Stratford, the Lords of the Admiralty or Encke's comet?

[Sir James South was at this time publishing in *The Times* an astronomical, semi-controversial correspondence with M. Boguslawski on Encke's Comet, and, in combination with Mr. W. S. B. Woolhouse, then late First Assistant in the Nautical Almanac Office, an attack on the Lords of the Admiralty and Lieutenant Stratford the Superintendent of the Nautical Almanac.]

unblemished people in the world : all they can show is, that they are unable to use the instrument, which I grant ; not that I am unable to use it, which I deny. Sir James blusters and shuffles, but dares not close with my offer. " He does his part extempore, for it is nothing but roaring."

As to Sir James's declaration that he has not read my letter, it is " strange, if true," and those may believe it who can. To answer what you have refused to read, might be thought inconsistent in another person, but it suits Sir James well enough. I have a better founded objection to his plan of " putting off his reply for some days owing to circumstances not under his control." First,—it is his own fault if he have vented a calumny, and at the end of a month has to seek for his justification. Secondly,—I believe the excuse to be a pretence, and the following story will show that I have good grounds for my belief.

Several years ago, Sir James South published a pamphlet, entitled *Charges against the President and Councils of the Royal Society*, from which this is an extract :—

" ADVERTISEMENT.

" It is well known to the scientific public that I stand pledged to present to them a work, entitled, *On the Conduct of the Royal Society, &c.* The unceasing attention, however, which the erection of my large equatorial has demanded will, I find, effectually preclude the completion of my wish. Aware of this, I have thought it advisable to publish forthwith a brief outline of those proceedings of the President and Councils, which will furnish the foundation of my future volume.

" JAMES SOUTH.

" *Observatory, Kensington, Nov. 11, 1830.*"

The pamphlet contained thirty-nine heads of accusation, and a conclusion, dressed up with notes, Latin quotations, signs of interrogation and exclamation, breaks, italics, and all that mute eloquence which certain authors delight and trust in. It was, however, kicked out of the room as unworthy of notice, and the author, having been extinguished in his attempt at agitation, " went thence and was no more seen," at least in that horizon. Now, though Sir James considered the facts contained in his charges to be " indeed appalling," and was thus doubly pledged by word, and triply provoked by the neglect shown to himself,

his pamphlet and the importance of his case, he has neither said nor sung, in verse or prose, in rhyme or reason, a redeeming syllable of confirmation. So much for Sir James South's promises ending in performance.

I again repeat that Sir James South, on his own showing, did not know how to observe Encke's comet. If the editor of *The Times* thinks the proof worth inserting (and, in conjunction with M. Boguslawski's letter it may have some utility,) I am quite ready to give it.

RICHARD SHEEPSHANKS.

Woburn Place, Nov. 17.

VI. November 20, 1838.—*Letter from SIR JAMES SOUTH to the EDITOR.*

SIR,—I have just, for the first time, read in *The Times* of the 5th inst. (for till now matters of far greater moment have occupied my attention) a letter addressed to me by the -Rev. Richard Sheepshanks. As, however, that reverend divine is a person with whom I must decline any direct correspondence, I beg permission to address to you what I have to say concerning it.

His reverence's letter, however, shall not draw me into matters which are at present before the legal tribunals of the country, any more than seems *absolutely* necessary to afford an antidote to the poison he wishes to convey into the public's stomach. A time, however, will arrive when, released from that restraint, I may with propriety publish to the world the manner in which that rev. gentleman has conducted himself during this procrastinated suit.

As, however, his reverence has thought fit to introduce the name of "Troughton" into this tissue of ribaldry, duncery, and misrepresentation, it may not be wholly irrelevant to state, that this lawsuit, though *nominally* that of "Troughton and Simms," is, in *reality*, that of the Rev. Richard Sheepshanks, commenced and carried on in their name and at their expense.

Troughton, my old and valued friend, is dead, and I regret to say (thanks to the Rev. Richard Sheepshanks) died not, as during the time I knew him, he had lived. I knew him well; and if

ever men were mutually attached, I believe I may safely say, *we* were.

The last time I saw him here as a friend (for subsequently his reverence brought him here as an enemy, and my heart almost bled to see him such) was in July, 1832, before I went to Russia, to see the large Russian equatorial, which I found as *perfect* as mine was *imperfect*, but after he had acknowledged that the large equatorial which had then been erected six months in my observatory, was "good for nothing;" on that day he begged me to take him a house, within a few yards of one of my own gates; named even the amount of rent and taxes for it within which I was to confine myself, saying, "for many years he had almost lived with me, and he wished to die as near as he could to me," and more earnestly than ever solicited me to "permit him to be buried in my grounds."*

Since he became estranged from me, very shortly after my return to England in November, reports have been circulated, and from a quarter which gave them the appearance of probability, "that his notorious partiality to me (for he would scarcely even work for any one else) was the result of pecuniary obligations to me, and that when he took Mr. Simms into partnership with him, he (Troughton) was in a state of insolvency." He never borrowed money of me in his life, and an intimate knowledge of his pecuniary circumstances (for he kept nothing from me) enables me to say that the last part of the report is a diabolical attempt to fix on him a fraud, at which I believe no human being would more have shuddered than would "Edward Troughton." During an uninterrupted friendship of nearly 20 years, I saw him generous even to a fault; his poor relations never asked of him and asked in vain; he was ever kind to his workmen, and during my intimacy with him I believe he never did an ignoble act.—*Requiescat in pace.*

But to proceed. My reverend libeller boasts of some observations of double stars, taken with the large equatorial by Messrs. Airy, Simms, and himself, omitting, however, the very important fact, that the very stars thus measured, and which are brought forward as a proof of the goodness of this worthless instrument (as if an equatorial was wanted for nothing else), had been measured by Sir John Herschel and myself, nearly twenty years

* In Dr. Maskelyne's lifetime, he expressed a wish to be buried in his garden, at the Royal Observatory, and even fixed on the site.

ago, with my small equatorial, the light-transmitting surface of whose telescope was not more than one-tenth, and its focal length little more than one-fourth of the telescope of the large instrument; or, in other words, these vaunted observations are as fit to demonstrate the perfection of the large instrument, even in the department of double star work, as would be the crushing of a worm to decide the power of an elephant.

As, however, his reverence modestly enough talks of settling a question between Struve and Bessel, as I know something more of these profound astronomers than he does, I will tell him, that whilst the one would laugh at his folly, the other would be ashamed of his support.

His reverence says, "It is two years since I saw your large equatorial, and the mounting of the telescope, when out of the meridian, did *not* then make any star a planet, or any nebula a comet. I speak as to the past from my own knowledge, and I do not believe that it does so now." As to what the rev. divine believes or disbelieves I care not one straw; his assertion, however, is a strong one: let us see if it be true.

Some three years ago, on my return from town, I was met in my grounds by a friend then on a visit to me, who had with the large equatorial discovered a comet. I was delighted to hear it, and we went to the instrument together. Sure enough there was some object in the field of the telescope, which had a very cometary look. He told me that when he first found the comet, he had placed it on the horizontal wire in the centre of the field, whilst now it had nearly moved away from it, to the field's circumference, a quantity far greater than the differences of refraction could produce. I brought the supposed comet again to the central wire, and on paper mapped its place relatively to some faint fixed stars which were visible in the field with it. I then laid the same body on the horizontal wire of the *small* equatorial, and determined its place by the divided circles of that instrument. The evening continued fine, and on returning to the large instrument, it was soon found that the supposed comet was only a nebula; for although it *left* the wire as before, its place amongst the stars, as laid down on the map, remained unaltered. Curiosity caused us before closing the dome of the small equatorial, to look into the telescope of that instrument, and there was the comet lying on the horizontal wire of the instrument nearly as we left it; it is very true the supposed comet at the time might have had

a motion so small in declination that an observation of it in right ascension might have been necessary with this instrument to have set the matter beyond a doubt; but that was not worth the trouble, seeing that by reference to the map such a motion existed not.

But lest this solitary instance should not suffice, let me add, that subsequently another friend of mine, with this same large equatorial, also during my short absence in town, discovered another comet; and I must say, produced such strong evidence of the fact, that had the instrument not been a most *mendacious* one, the discovery would have been indisputable. To confirm or disprove the supposed discovery, we recurred to the mapping process. The suspected body in this instance was too faint to be perceived with the small equatorial; on our return, however, to the large equatorial, although the comet had nearly left the field, its position amongst our mapped stars remained just as we first placed it. Nor were the observers men of ordinary observing skill—the one of them is far above mediocrity, whilst the other, by the late Bishop Brinkley, was held “second to *no* astronomer of Europe.” I need not name him. His reverence’s strong assertion, then, that when he saw the instrument two years ago it did *not* then make any star a planet, or any nebula a comet, being not a very reverential one, I will not comment on—perhaps the public will.

Now to another paragraph, by which he fondly hopes to gull the public—viz., his congratulation “that I was able to sweep for, find, identify, and ascertain the place of Encke’s Comet with this much-abused equatorial.” I know not whether ignorance or “malevolence,” or both, helped him to this congratulation. Will it be believed that any man having the slightest observatorial knowledge would adduce the finding of Encke’s comet as a proof of the perfection of an equatorial instrument—not knowing, as if he be sincere he evidently does not, that such objects can be seen tolerably well in telescopes which are incapable of showing *anything else* with distinctness? Comets when first visible, are diffused patches of light, occupying a considerable space in the field of the telescope, and, like nebulae, are well seen in the field of a telescope, even though agitated somewhat by the wind, nearly as well as in the finest equatorial in existence, and under cover of a dome. As a proof, however, that to find nebulae even of the faintest class, and therefore comets also, no equatorially-mounted

telescope is needed, let me state the fact, that not one of the many thousand nebulae discovered by Sir William Herschel were discovered by one so mounted; indeed, I think on one occasion, when he regretted he could not, from bodily infirmity, go to Blackman Street, to see my instruments, with which his son and I were then observing, he told me he had never looked through an equatorial in his life. Perhaps, however, his reverence forms his ideas of other people's abilities to find comets without equatorial telescopes by his own. Let me then tell him, "Sir William Herschel" could place his 20-feet telescope on any star in the heavens then above the horizon, knowing its approximate right ascension and declination only within five minutes of the time he had made himself acquainted with those elements. With the large telescope now belonging to the large equatorial, when it was on its temporary mounting and (which was not equatorial) in front of my house, I could do the same; and I will tell his reverence, that on this stand, made at not one-fiftieth of the expense of the large equatorial mounting, I should have found the comet just as certainly, and just as early as I did. I might go on to say, that it was by following the system of mapping that Harding discovered the planet Juno with a telescope scarcely able to show me well even the satellites of Jupiter, mounted on a stand which I think he told me cost about 2*s.*, whilst the illustrious Olbers, by this mapping process, with an object-glass, whose effective diameter I found to be barely two inches, discovered Pallas and Vesta, the telescope's stand being nothing but his hand. Let me then tell the reverend divine, if an equatorial be good, such as my small one, made now nearly fifty years ago, mapping is unnecessary; but if it be execrable, like the large one, then he is a dunce, be he who he may, who would neglect it.

As to the "fictitious motion of 11 minutes of a degree, which the telescope gives to any celestial body in the field," his reverence's special pleading shall not serve him, for I will transcribe the following note, written by an observer, as it stands in the journal of September 23, 1835:—"Set the telescope to 0 degrees 0 minutes 0 seconds, and turned the polar axis round about four hours in right ascension, and the reading had then altered 11 minutes of a degree." Here follows the signature of the observer.

As to his reverence's determination of the instrument's ordinary error being "between 30 and 40 seconds of space," seeing it differs so widely from that arrived at by the most accomplished astro-

nomer of Great Britain, I must be permitted to doubt whether his reverence read the verniers rightly, or, having read them rightly, whether he has not reduced them wrongly.

Now to his reverence's last proposition, introduced (not inappropriately) with a *nota bene*! In the early part of this communication, I hinted that but for his reverence's interference I should have had no law. Those who know him not might think that the writing me insolent letters signed "Troughton and Simms," the excluding me out of my own observatory for six or seven months, while he was spending for me, or "Troughton and Simms," an additional 200*l.* in botching up with divers fooleries a thing not worth as many farthings—selecting the witnesses brought against me—in short, the getting up the case against me, might have satisfied him. But no! his object *per fas nefas* was "TO SMASH ME!" Truth, however, seemed dangerous to his project, and he even dared, as in the instances of Mr. Jones, Captain Beaufort, and of Professor Babbage, to threaten with his vengeance those who came for me to swear it.

A wager may be "a bait" to the gambler and the blackleg, I wish to rank as neither; on that ground only, although offered by a *clergyman of the Church of England*, would I scornfully reject it.

Again, Professor Babbage, one of my earliest and one of my most esteemed scientific friends, as well as Captain Beaufort, has suffered insult for daring to be honest in my cause. Sir J. Herschel, the first of his reverence's referees (a right noble office truly), with whom, during some years, whilst a frequent inmate of my house, I passed some of the pleasantest part of my life, shall on no account be placed in a similar predicament.

In addition, however, to these, other reasons, "*quæ nunc prescribere longum est*," cause me thus publicly to tell the Rev. Richard Sheepshanks, A.M., Fellow of Trinity College, Cambridge, that never whilst these premises are mine shall *he* again set foot within them.

Your obedient servant,

J. SOUTH.

Observatory, Kensington, Nov. 17.

P.S.—On Monday, Nov. 5, a letter was left for me at the Athenæum Club-house (of which the Rev. Richard Sheepshanks

and myself are members) impressed with a seal resembling (I believe) that of the Athenæum Club-house; unable there to ascertain the fact whether written by him or not, I brought it home, and, on comparing it with some of his reverence's notes of former times, telling me he "should be very glad to come and dine with us," I no longer was left in doubt. Of course I did not open it, but, inclosing it in an envelope, fixed on it my own seal, addressed it with my own pen, and sent it by the post to his reverence's residence, 30 Woburn Place, Russell Square, where I trust it has arrived safe.

VII. December 4, 1838.—*Letter from the Rev. R. SHEEP-SHANKS to the EDITOR.*

[Advertisement.]

SIR,—On Friday last I forwarded to *The Times* a full refutation of Sir James South's letter to me (Nov. 20th) and a demonstration of his incapacity as an observer. This I am told can only be admitted as an advertisement. I cannot afford to pay for the privilege of communicating knowledge, and the editor, as far as I am concerned, is welcome to prefer Sir James South's "*old mumpsimus*" to my new *mumpsimus*" in all astronomical subjects. In return, it will be but reasonable to suppress his extraneous and offensive remarks, if they may not be answered except at a ruinous expense.

Sir James has mentioned "some reports" favourable to his own pecuniary liberality and discreditable to Mr. Troughton's integrity, which, with great generosity, he contradicts. Have these reports (supposing them to exist) any deeper origin than Sir James's inventive faculty? Using the Cassian test of "*cui bono fuerit*," I can guess at no other author; yet this would surpass the marvels of the "kennel and conversation"* day. Sir James must produce his informant or submit to the imputation.

The letter which Sir James South returned to me unopened was to remind him of a speech which he seems to have forgotten, though it was made upon oath and in the presence of his attorney,

* See Sir J. South's letter, November 17th, and Lieutenant Stratford's unanswered reply, November 21st.

&c. He proposed, "that he and I should be shut up in a room together and examined in astronomical calculations," and I accepted the proposal. Now, I protest, that I am, and always have been, ready and willing to fulfil my part of this peaceable duello, and if the affair hangs off any longer, that Sir James is the backer-out, not I. Tables of logarithms, nautical almanacs, two policemen, and one strait waistcoat will be sufficient paraphernalia.

The services of a military officer of high rank will not be required.

R. SHEEPSHANKS.

[The last paragraph of this letter refers to a letter from Sir James South to the Editor, published November 29, 1838, in which the writer implies the assertion that a certain letter, sent by him to Mr. Airy, the Astronomer Royal, was intended to provoke Mr. Airy to a duel! The words which contain the implication are as follows:—"This letter having been put into the post by a friend, I waited on a military officer of high rank in his majesty's army, who kindly undertook to receive for me any communication with which I might be honoured by Mr. Airy, and to make on my part such arrangements as might be necessary. J. S."]

VIII. December 5, 1838.—*Letter from Sir JAMES SOUTH to the EDITOR.*

[Advertisement.]

SIR,—The first paragraph of the Rev. Richard Sheepshanks' letter to you published in this day's *Times* is distressing enough. The third is funny enough, but would have been more so had his reverence finished the story. The fourth is sarcastic enough, considering that Mr. Airy, against whom it is directed, is his reverence's most intimate friend. Whilst the second is so admirably adapted to exclude me from all decent society, that I shall here present the reader with a copy of it:—

"Sir James has mentioned 'some reports' favourable to his own pecuniary liberality and discreditable to Mr. Troughton's

integrity, which, with great generosity, he contradicts. Have these reports, supposing them to exist, any deeper origin than Sir James's inventive faculty? Using the Cassian test of '*cui bono fuerit*,' I can guess at no other author; yet this would surpass the marvels of the 'kennel and conversation' day. Sir James must produce his informant or submit to the imputation."

Politeness to a rev. clergyman of the Church of England not permitting me to allow his query, "*cui bono fuerit*?" to remain unanswered, I must state that the "Messrs. Troughton and Simms," whom the reverend divine has contrived to get into litigation with me, were persons to whom during many years I showed kindness of no common order—occasioning the former to regard me almost as his son—whilst by the latter, to use his own words, I was looked upon as having been "more than a father."

These facts were well known to the various opticians and astronomical instrument-makers of this metropolis; and on it being spread amongst them that they (Messrs. Troughton and Simms) and myself were about to "go to law," their ingratitude towards me was freely talked of. A remedy for this was, therefore, wanting, and pseudo-poietic ingenuity soon supplied the following (given in the sixth paragraph of my letter to you published in *The Times* of November 20)—namely, "that Mr. Troughton's notorious partiality to me (for he would scarcely even work for any one else) was the result of pecuniary obligations to me, and that when he took Mr. Simms into partnership with him, he (Troughton) was in a state of insolvency."

Such, then, were the reports, and more unfounded ones were never uttered; as reports they stand, however, and on my assertion, and on my assertion only; unwilling, therefore, that they should remain so, this afternoon I called on my informant, who gave me a document, of which the following is a copy:—

"For the reports alluded to in the 6th paragraph of Sir James South's letter published in *The Times* of November 20th I am Sir James South's authority; and they are nearly word for word what occurred, as far as I remember, in a conversation which I had with Mr. Frederick Simms in the year 1833.

"GEORGE CARY.

"*St. James's Street, December 4, 1838.*"

Now this "Mr. Frederick Simms" is "Mr. Troughton's partner's brother!"

Thus, then, there is another "man of truth" waiting for the pillory of public execration. When will his reverence's stock of these *animals* be exhausted?

J. SOUTH.

Observatory, Kensington, December 4, 1838.

IX. December 6, 1838.—Sir JAMES SOUTH to the EDITOR.

[Advertisement.]

SIR,—In the fourth paragraph of my letter, published in this day's *Times* several words I inadvertently omitted, and, as the omission renders a very important part of the letter pointless, let me beg permission to say that the sentence beginning "A remedy," ought to have been "A defence against this on the part of Mr. Simms (who, from being 'a workman in the garret of an obscure lodging-house,' had, thanks to me and his own then good conduct, been taken by Mr. Troughton without premium as his partner), was therefore wanting," &c.

J. SOUTH.

Observatory, Kensington, Dec. 5.

X. December 6, 1838.—The Rev. RICHARD SHEEPSHANKS to the EDITOR.

[Advertisement.]

SIR,—Before replying to Sir J. South's advertisement, I must remind my readers how our dispute now stands.

Sir James South, in speaking of Encke's comet, made certain statements respecting his large equatorial, which I contradicted, offering at the same time to prove myself right by a reference to the instrument, without occasioning expense or trouble to him. This he refused, upon pretexts evidently evasive and ridiculous. I therefore ask to have judgment in my favour.

About two years ago, Sir James being then on his cross-examination, voluntarily proposed, that "he and I should be shut up in a room together, and examined in astronomical calculations," and I accepted the proposal. When I remind him of his former gallantry, and press him to put it to proof, he gives no answer. Does Sir James imitate "the foolish knight of Illyria," who thought it "an excellent jest to challenge his adversary to the field, and then to break promise with him?" or does he conclude with the same sapient Sir Andrew, "that if he be to redeem himself, and't be any way, it must be with valour, for policy he hates?" I shall take the liberty of speaking (astronomically, of course) very disrespectfully of Sir James, if he copy Sir Andrew so closely.

Again, Sir James made a charge of "malevolence" against me, wholly unprovoked, and in my absence. The circumstances with which he connected his charge, I showed to be false and foolish from his own language. He now attributes to me his disagreeable situation as defendant at the suit of Messrs. Troughton and Simms. Now he knows that I never interfered in the family quarrel until he had broken, irrevocably, with his "father Troughton" and his "son Simms." He refused to pay their bill, and wrote very undutiful letters. He was the first to introduce an attorney into the affair, and that attorney begged of Messrs. Troughton and Simms to name their attorney. In aiding a just cause I had motives which perhaps would not be intelligible to Sir James South.

Sir James talks much and largely of the obligations of Messrs. Troughton and Simms to himself. These appear to me of a reciprocal nature. Mr. Troughton wrote for Sir James the descriptions of instruments for his memoirs, gave him his countenance, and taught him the little that he knows of practical astronomy. Sir James most filially dressed himself in Troughton's knowledge, pillaged and potioned him in sickness, gave him dinners in health, and flattered him in and out of season. Sir James recommended Mr. Simms, and took out the value of his patronage in cheap work, some of which he sold to an enormous profit. I never heard Mr. Troughton or Mr. Simms speak of Sir James being "almost a son to the one," and "more than a father" to the other. Such fustian is more suited to the latitude of Campden Hill than to Fleet Street. When Sir James's poetical and metaphorical descriptions are reduced to plain prose, the facts are as I have

stated them. He ordered an instrument, then would not pay for it, they bring their action, and I help them.

The only party interested in spreading reports injurious to Mr. Troughton or Mr. Simms and favourable to Sir James's generosity is clearly Sir James, and accordingly I demanded his informant, proposing the Cassian test. Sir James produces Mr. Cary, who says he heard the reports "word for word, as far as he remembers, from Mr. Frederick Simms in 1833." I have applied to Mr. F. Simms, who is out of town, but I admit that Sir James stands quite clear from the suspicion of invention; he has only revived and given publicity to false and scandalous reports, after five years' repose and neglect, for his own glorification, and *à propos* of nothing at all. So far I acknowledge Sir James's merits, but I cannot decipher his meaning (and, in passing, let me advise him to eschew Greek compounds, for he thereby says what he does not intend) when he wishes to show that in 1833 Mr. Troughton (then alive), Mr. Simms, or I, had any interest in libelling one of our own party, or in praising Sir James's unheard-of generosity.

Whether Mr. F. Simms has misinformed Mr. Cary, or Mr. Cary misunderstood Mr. F. Simms, those gentlemen, after five years, may find it difficult to settle. I would remind Sir James that after his report of an interview with Lieutenant Stratford before a witness, and his subsequent conversation with that gentleman, has been so circumstantially contradicted (in which state it now remains), he is too hasty in invoking the pillory of public indignation on a one-sided statement, and also that recollections of conversations are ticklish things to rely upon after the lapse of several years.

R. SHEEPSHANKS.

XI. December 7, 1838.—SIR JAMES SOUTH to the EDITOR.

[Advertisement.]

"Ecce iterum Crispinus."

SIR,—Since the Rev. Richard Sheepshanks's letter appeared in *The Times* of Tuesday last I have indeed been wretched; for it announced, that owing to the low state of his reverence's exchequer, that relish for my breakfast which his communications to you could alone produce was from that date to be withheld.

Indeed, enjoying as I do a hearty laugh, nothing save respect to "that order" of which his reverence stands at once the "*decus ac delicia*," deterred me from offering to pay for his reverence's advertisements as well as my own.

Judge, then, Sir, my delight when, whilst eating my breakfast this morning, *Times* in hand, I found that "Richard was himself again!"

For this important service rendered me, I would pour out my gratitude, did I but know where gratitude was due. As, however, it is doubtful by whom his reverence's petition of Tuesday last, "*Pray remember the poor parson*," has been responded to, I am deprived that pleasure.

It is well known to my friends that it has *never* been my intention to allow his reverence or his reverence's friend, the Lieutenant, to escape for their various slanders, misrepresentations, &c., in *The Times* of November 19 and 21, without that grilling which they merit; although, from his reverence's impatience to receive it, we may say of them, as did the fish-woman of the eels, which she was skinning, "Lord, Sir, it's nothing to them, when they are used to it!"

Assuring, then, his reverence and his nautical friend, that so soon as I can rout out the necessary documents, they shall hear from me again,

I remain, Sir, your obedient servant,

J. SOUTH.

Observatory, Kensington, Dec. 6.

P.S. A facetious friend has just remarked, that he perceives "his reverence and the sapient lieutenant have hired a gig between them." *Verbum sat sapienti.*

[The last two paragraphs of this letter, and a paragraph in that which precedes, contain references to a letter of Lieutenant Stratford to the Editor of *The Times*, published November 21, 1838, in which he distinctly contradicted the account given by Sir James South of an interview between them, and challenged Sir James South to bring forward his own solicitor, the only other person present. The answers promised by Sir James South in the preceding letter never appeared.]

XII. December 8, 1838.—*Letter from the Rev. RICHARD SHEEPSHANKS to the EDITOR.*

[Advertisement.]

I have received the following note from Mr. Frederic Simms, relating to the reports said to have arisen on his authority (see Sir J. South's letter, *Times*, Dec. 5):—

“Five years ago, I may have said to Mr. Cary that my brother's partnership with Mr. Troughton was, in a pecuniary point of view, a losing concern; indeed, it was so for five years out of the seven, but I do not recollect to have said even this.

“I am certain that I never spoke of Mr. Troughton as being under pecuniary obligations to Sir J. South, and until Sir J. South's letter appeared, I never heard of such a report.

“F. W. SIMMS.”

“Dec. 7, 1838.”

So much for conversations, held five years ago, and kept in Sir James's journal for a convenient opportunity. The small gear of gossip, reports, inuendoes, &c. (flavoured by Sir James, and dignified by the name of documents), has been my opponent's food for many a year, and the effects on his *pia mater* have been deplorable.

Sir James says not a word of his challenge to me in the field of astronomical calculation. For a knight of lion-like language, who “lets his courage appear when there is no need of such vanity,” never was any one harder to bring to the proof than Sir James South. “He is a good drum, but a naughty” astronomer; or, perhaps “his discretion carries his valour as the fox carries the goose.” Swaggering is his forte, not computing.

Sir James utters some threats for the future, and gives hints of his generosity and merriment; but, as I have answered every issuable matter in his letters, I here bring the debate to a close. I shall have something more to say to Sir James in a little time, and on his own business.

RICHARD SHEEPSHANKS.

Extract from Chap. XII. headed 'Intrigues of Science,' of "the Exposition of 1851, a View of the Industry, the Science, and the Government of England. By CHARLES BABBAGE, Esq." (2d Edition, London, 1851, 8vo. pp. 154-172.)

§ In a work containing views on the state of science in England, foreigners at least will expect that I should take some notice of my own calculating engines.

I had hoped that the history of the transactions between myself and the government respecting them, as related in the eleventh chapter of the History of the Royal Society by Mr. Weld, together with the two criticisms on that work in the *Athenæum*,* would have rendered any further explanation on my part unnecessary. Many persons, however, who admit these as fully explaining the part I was compelled to take, have at the same time expressed to me their doubts that some occult agency was at work to prejudice the government, and have asked who were its scientific advisers on such an important subject, during the long period in which the Difference Engine was in abeyance.

§ I have not been blind to the passions and interests of men. My own pursuits were of such a character that they interfered with those of none of my colleagues in the paths of science; and perhaps I may have trusted too much to this circumstance as exempting me from rivalry and jealousy.

As a reformer both in science and in politics, I knew that I should excite enmity in the minds of some honest men, and also in those of many other persons who dreaded inquiry into jobs not yet exposed. When I published the *Decline of Science*, in 1830, I certainly was not aware how many would include themselves in the latter class: but had I foreseen it, I should not have altered my course. To have met and to have defeated intrigue by watchfulness, might not have been a difficult task, but it would have required too great a sacrifice of time devoted to far higher objects. It was, moreover, an occupation for which I had little taste.

The time, however, has now arrived when, having given up all expectation of constructing the Analytical Engine from the drawings which I had caused to be made at very great expense, I think it right to state the result of my own observations, and

* *Athenæum*, 14 Oct. 1848, and 16 Dec. 1848.

especially to point out the facts that have come to light to confirm them. These, if they do not open the eyes of some, who, having been themselves deceived, have done me injustice, will at all events be of use for the future, and may save the young and inexperienced enthusiast of science from embarking in undertakings, honourable to the country, but ruinous to himself.

It has often been remarked, that an event in itself trivial sometimes leads to results with which it seems to have no conceivable connexion.

A beaver constructing his dwelling on the plateau of the Andes, may have turned the course of a river, which otherwise would shortly have joined the Pacific, into a valley through which, after lengthened wanderings, it now flows into the Atlantic Ocean.

So, by some strange combination of circumstances, a quarrel in which I had no part, and with whose origin I am unacquainted, seems to have had an unanticipated effect in impeding the construction of the Calculating Engines.

At the time of the foundation of the Astronomical Society, Sir James South, whose observatory and whose house were hospitably open to every cultivator of astronomy, was on terms of intimate friendship with almost all of those persons at that period most eminent in science. It is sufficient to mention the names of Wollaston and Davy; and to add that when the late Mr. Fallows was appointed Astronomer at the Cape, although previously a stranger, he became for several months the guest of Sir James South, who assisted him in acquiring that practical knowledge of instruments so necessary in his new avocation.*

§ In 1829 Sir James South was elected President of the Astronomical Society. It now appears, however, that previously to this appointment, *a party had been formed* adverse to Sir J. South, which party, with the view of thwarting him, placed in the office of Secretary the Rev. Richard Sheepshanks, Fellow of Trinity Coll., Cambridge.†

In March, 1831, the Board of Visitors of the Royal Observa-

* Sir James South, in conjunction with Sir John Herschel, completed the examination of 380 double and triple stars; a work for which the authors were awarded the great Astronomical prize of the Institute of France in 1825, and the Medal of the Astronomical Society of London in 1826.

† "When he [Sir J. S.] was elected President, I [Rev. R. S.] was elected Secretary to keep him in order."

tory of Greenwich, met at the Admiralty, to consider the propriety of separating the duties of Superintendent of the Nautical Almanac from those of Astronomer Royal. The new arrangement was advocated, amongst others, by Sir J. South, and after some discussion, in which Capt. Beaufort and myself took part, it was ultimately carried. As we were leaving the meeting-room, Mr. Sheepshanks addressing me said: "I am determined to put down Sir James South, and if you and other respectable men will give him your support, I will put you down." He at the same time told me he "intended to put Captain Beaufort down."

During the course of 1832, it was found that the large equatorial mounting which had been contrived and executed by Troughton, for his friend Sir J. South's twelve-inch object-glass, was an entire failure. This produced at the time a difference between two friends who esteemed each other highly, and who had been for years united by reciprocal acts of kindness in ties of "*very intimate*" friendship. Well acquainted myself with the character of the parties, and the circumstances of the case, I have not the slightest doubt that this unfortunate affair might easily, by the exertions of judicious friends, have terminated in the entire restoration of their former friendship. But this was a course which the Rev. R. Sheepshanks took effectual means to prevent. Having himself a "*personal*" quarrel with Sir James South, he "*offered*" his services to assist Messrs. Troughton and Simms. He "*offered to go*" himself to examine the instrument in Sir J. South's observatory, and "*got his friend, Professor Airy, to go with him*" for the purpose of remedying the defects of the Equatorial.

Notwithstanding he was told by Mr. Simms that "*Sir J. South had declared that no person could have been pitched upon more obnoxious than yourself*," he still persevered in obtruding himself into Sir J. South's observatory as the agent of Troughton and Simms, until it was at last discovered that no after contrivances or expense could correct the errors of an instrument itself radically defective in principle.

It may readily be supposed that the continuance for months of these visits by Mr. Sheepshanks and Professor Airy, and the *irritating correspondence* consequent upon them, which, though nominally that of Troughton and Simms, was really "*directed by*" the Rev. R. Sheepshanks, destroyed all hope of a reconciliation. The parties then had recourse to the Court of King's

Bench, and it was curious to observe the vigour and energy with which the Rev. R. Sheepshanks applied himself to the exercise of his earlier studies.*

Having *volunteered* his services to Messrs. Troughton and Simms—he “*wrote every letter*” for them during the subsequent lawsuit—he acted for them in all the various characters of “*friend*” and “*adviser*”—of “*workman*” and “*agent*”—of “*attorney*” and “*counsel*,”†—he made an “*affidavit*” in the case—became a *witness* himself—and undertook to *intimidate witnesses* on the opposite side.

This latter performance is fortunately rare in England, and is so remarkable that it is necessary to give some account of the proceedings.

Not wishing to become involved in so disagreeable a case, I had refused to be a witness on the part of Sir J. South. Having, however, had some conversation on the subject with the late Lord Abinger (then Mr. Scarlett), he represented to me that my evidence was essential for the justice of the case, and upon that ground I reluctantly waved my objection to appear as a witness.

Having been examined in chief on the seventeenth day of the Arbitration, I remained in the room a few minutes after the Arbitrator had left it. The Rev. R. Sheepshanks, the only other person then present, addressing me said, “it was necessary to “*discredit me* because I had supported Sir J. South.” He added that “he would, at a future time, *attack me* publicly on *another subject*, on account of the part I had taken in this matter.”

The remembrance of his former threats more than four years before at the Visitation at the Admiralty, added to the knowledge of the unremitting perseverance with which he had carried on his hostility to Sir J. South, satisfied me that it would be unsafe for the cause of truth, and possibly injurious to myself, if I were not to take measures for making known the nature of the weapons which the Rev. R. Sheepshanks was employing. As he had ventured, *after* my having given evidence on oath, to threaten me with injury, with the hope of inducing me to modify that evidence on cross-examination, it appeared to me probable that he might

* At an earlier period of his life, his studies were directed towards the profession of the law.

† On the 19th July, 1836, at the 23d meeting under the Arbitrator, the Rev. R. Sheepshanks *cross-examined* Mr. Savage the Architect.

have been tampering with the evidence of other witnesses in the same cause, who from their position or circumstances in life, might be compelled by the fear of his vengeance to shape their evidence so as to adapt it to his views.

The Rev. R. Sheepshanks discovered on reflection no impropriety in this course of intimidating witnesses, or of attacking those who could not be induced to take up his own private quarrels. He thus defended both.

"I think it allowable to throw down the gauntlet in this manner."

"I have another ground of dispute with Captain Beaufort, and certainly intend to put him down."

The gallant Admiral has survived many a dangerous day, and needs not the pen of a friend to protect his honest and well-earned fame.

The reader may perhaps be astonished at the statement made in the preceding pages, and feel disposed to consider it an *ex-parte* statement. It is *entirely* an *ex-parte* statement: it is not necessary for its support that the reader should give credence even to that small part of it which appears to rest on my own evidence before the Arbitrator. *The whole of it is founded entirely on the testimony of the Rev. R. Sheepshanks himself.* Every statement of those which are marked as quotations was either elicited from him on his cross-examination, or in the few instances in which it came from myself, its correctness was confirmed by his subsequent admission or re-statement. After my statement, and the Rev. R. Sheepshanks' reply to it, the Arbitrator addressing him said,—

"With respect to the matter of fact, you agree?"

Rev. R. Sheepshanks. "Yes, we agree as to the matter of fact."

Professor Airy, who was afterwards appointed Astronomer Royal, had long before become as deeply engaged as his friend Mr. Sheepshanks in this most unfortunate quarrel. Years of aggravating delay and discussion resulted from the procrastinated reference, and at length one of the parties, Mr. Troughton, being dead, a decision not satisfactory to either was given in December 1838. But the inextinguishable desire "to put down Sir James South" survived the lawsuit which was only used as a means, and reappeared from time to time through the aid of the press, in forcible but somewhat unmeasured charges and recriminations

between the Astronomer Royal, the Rev. R. Sheepshanks and others on the one side, and the astronomer of Campden Hill on the other.

It was a curious, though a very painful study, to observe from time to time the various consequences of this feud.

Against those men of science who refused to forsake their ancient social relations with Sir James South, a system of disparagement was maintained which could not fail in the course of time to produce its effects. The avowed object of the party of which the Rev. R. Sheepshanks was the organ, was, in his own expressive words, to *discredit and put down every respectable person* who supported Sir J. South.

It was melancholy to observe the gradual change in the expression of opinions by some of those qualified from their knowledge to guide the opinion of the public. Intimidated at first into silence; the uncontradicted assertions of those around them then got possession of their minds, until at length, without any new examination, they were flattered into an acquiescence in, if not indeed into the expression of, opinions entirely opposite to their former ones. These new views were doubtless conveyed by their flatterers to other ears, and thus the process of "*discrediting every respectable person*" opposed to them, was carried on under the authority of honourable names.

One after another almost all Sir James South's old friends and acquaintance, amongst *men of science only however*, were alienated from him.

One man was alarmed by the fear that some inaccuracies in his astronomical publications should be severely criticised. Of another it was hinted that his mathematics were all wrong, and might be shown up.

Those who were timid feared the anger of the dominant party; those who were young might have their prospects blighted by even appearing in friendly relations with him who supported the unequal conflict; those who were old loved repose, and found it easiest to appear to side with the most numerous party; whilst those who saw through the whole of it, had better things wherewith to occupy their minds, than to attend to such affairs.

It is obvious to all who have observed society that such a system of "*discrediting*" carried on for a series of years, especially against one too much occupied or too proud to expose it, must end in establishing the set of opinions propagated by the party.

Honest and even tolerably well-informed persons, will at length be misled, and be found to adopt them.

Opinions thus propagated must have had their influence widely spread, and unless those members of the various administrations with whom decisions relative to the Difference Engine rested, had been either highly skilled in mathematical science, or deeply read in human nature, it would have been almost impossible for them not to have been misled.

The former qualification is unnecessary; the latter is indispensable for a statesman. Of the *eight* Prime-ministers with whom I have had communications relative to the Difference Engine, *one* only personally examined it; doubtless not with the view of criticising the mechanism, but of reading the character of its author. Had my *official* intercourse with that eminent man commenced earlier or continued later, the fate of the Calculating Engines would probably have been far different.

It is always difficult to trace intriguers up to a direct intercourse with Government. In the present case, the vanity of some of them overcame their judgment, and they gave themselves out as advisers of the Government on scientific subjects. To these I shall not at present refer, but confine myself to citing from official documents two cases of direct communication with the Government by persons on whose judgment it appears to have relied.

The Whigs seemed to have had great confidence in the devotion of the Rev. R. Sheepshanks to their interests, since they took the extraordinary step of appointing him, although a Clergy man, one of the Boundary Commissioners under the Reform Bill and he is, I believe, at present one of the Standard Measure Commission.

The Astronomer Royal, besides his situation at Greenwich, has been a member of several Commissions:—

- The Tidal Harbour Commission.
- The Standard Measure Commission.
- The Harbour of Refuge Commission.
- The Railway Gauge Commission.

The following are extracts from his Annual Reports:—

“The Board of Admiralty, on my representation of the interruption to our business caused by the rating of so many Chronometers, and *by my own employment on public business unconnected with the Observatory*, immediately sanctioned

the employment of an additional computer."—*Astron. Royal, Rep. June 1841*, p. 7.

"On former occasions I have avowed without scruple that I do not consider the Royal Observatory as a mere isolated place for the conduct of Astronomical observations. I consider it a part, perhaps the most important part, of the scientific institutions of this country."—P. 18.

"In concluding this long report, I have been uniformly supported by the confidence of the Government."—*Astron. Royal, Rep. June 1844*, p. 20.

The following extract of a letter from the Astronomer Royal to the late Sir Robert Peel, shows that his time was so occupied with the labours of the Railway Gauge Commission, that he was unable to draw up a memorial which he had himself proposed, even though it related to an astronomical subject—our colonial observatories.

* * * * "I have been so closely employed on the papers of the Railway Gauge Commission, that it has been impossible for me to draw up a memorial before the present time. * * * *

"April 16th, 1846.

"To the Right Hon. Sir Robert Peel, Bart. &c."

"By the giving opinions on subjects of railways and *other mechanical matters referred to me by Government*, it has appeared that our energies are not wholly absorbed in the mere Astronomy of the Observatory."—*Astron. Royal, Rep. June 1846*, p. 10.

(N.B. The italics do not occur in the original quotations.)

Now it is evident from these extracts from Reports of the Astronomer Royal to the Board of Visitors and from other facts, that he wishes himself to be considered the general referee of Government in all scientific questions.

The office of Astronomer Royal is one of great importance: it requires the undivided energy and talents of one person, and great as Mr. Airy's abilities undoubtedly are, yet it is highly injudicious to divert them from their legitimate object,—the direction of the many arduous duties of the establishment over which he presides.

During many years I have frequently found, in my communications with members of government on subjects connected with the Calculating Engines, difficulties on their part which remained entirely unexplained;—unseen obstacles which were never alluded to, but whose existence could not be doubted.

Although frequently warned by personal friends that it was unwise to neglect such machinations as those which I have, at

length, been reluctantly compelled to expose; yet I was unwilling for a long time to believe that they were directed against myself.

I have now traced the connexion of the Rev. R. Sheepshanks (who had avowed his determination "*to discredit me,*" and also "*attack me on another subject at a future time,*"), through his friend the Astronomer Royal, with the Government. According to the Astronomer Royal's own statement, he was their adviser on all scientific subjects. The Government had no other official adviser, and would scarcely have ventured to decide upon points connected with some of the most profound questions of mathematics, on their own responsibility.

There are, I am aware, other channels than those of official reports, by which the Government may have been influenced. I do not, therefore, expect to find any formal report denying the practical utility of the Calculating Engines, or the possibility of constructing them.

If there is any such, I claim as a matter of justice, that it be published. The Difference Engine and the Analytical Engine are questions of pure science. If the Astronomer Royal has maintained that they are either useless or impracticable, then the grounds of that opinion *must* have been stated, and, if published, the solidity of those grounds might be examined.

It now becomes necessary to take a very brief review of the conduct of Government with respect to the Difference Engine. Having contrived and executed a small model of a Difference Engine, I published a very short account of it in a letter to Sir Humphry Davy, in the year 1822. At the wish of the Government I undertook to construct for them an engine on a much larger scale, which should print its results. I continued to work at this Engine until 1834, refusing in the meantime other sources of profitable occupation, amongst which was an office of about 2500*l.* a-year. Circumstances over which I had no control then caused the work to be suspended.

After eight years of repeated applications, and of the most harassing delay, at the end of 1842 the Government arrived at the resolution of giving up the completion of the Difference Engine, on the alleged ground of its expense.

In the meantime, new views had opened out to me the prospect of performing purely algebraic operations by means of mechanism. To arrive at so entirely unexpected a result I deemed

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A Letter

TO THE

BOARD OF VISITORS

OF THE

GREENWICH ROYAL OBSERVATORY

IN REPLY TO

THE CALUMNIES OF MR. BABBAGE AT THEIR
MEETING IN JUNE 1853, AND
IN HIS BOOK ENTITLED *THE EXPOSITION OF* 1851.

BY THE

REV. R. SHEEPSHANKS, M.A.

ONE OF THE VISITORS.

Lædere gaudes,
Inquis ; et hoc studio pravus facis. Unde petitum
Hoc in me jadis ? est auctor quis denique eorum
Vixi cum quibus ?

LONDON :

PRINTED BY G. BARCLAY, CASTLE ST. LEICESTER SQ.

1854.

*" Chi potendo vietar, non vieta il male,
È partigian della ribalderia ;
E chiunque è gentiluom naturale,
È obbligato per cavalleria
D' esser nemico d' ogni disleale,
E far vendetta d' ogni villania ;
E se qualch' un di voi questo disprezza,
In se non ha bontà nè gentilezza."*

BERNI, *Orlando Innamorato*, lib. i. canto xxi. stan. 6.

ADVERTISEMENT.

SOME time after the opening of the *Great Exhibition of 1851*, Mr. Babbage published a work, entitled, *The Exposition of 1851 ; or, Views of the Industry, the Science, and the Government of England*. The twelfth chapter of this very miscellaneous jumble is headed, *Intrigues of Science*, and is chiefly devoted to a relation of the persecutions of which Mr. Babbage imagines himself to have been the object, and of which he supposes me to have been the director and manager.

A few months later, Sir James South, who is Mr. Babbage's intimate ally and tool, published a letter in the *Mechanics' Magazine*, in which he records certain conversations between himself and the late Mr. Troughton, 'about thirty years ago. I have some grounds for suspecting this publication to have been *instigated* by Mr. Babbage.

I did not hear of Mr. Babbage's attack upon me till some time after its publication, and it was in conversation with Mr. Airy that I first learned its nature. I was too much engaged at the moment to make a reply, and felt in no hurry about it, being assured that no *well-informed* person could give credence to such a tissue of absurd assertions, and still more absurd deductions. Perhaps I should have passed the matter over altogether, as not worth my attention, if Mr. Babbage had not sought a further occasion for discharging his spleen.

Having obtained, as he supposed, a sufficient *corpus delicti* in Sir James's published letter, Mr. Babbage took upon himself the grateful and congenial task of public prosecutor. He sent copies of the *Mechanics' Magazine* to the Councils of the Royal and of the Royal Astronomical Society, as a sort of impeachment ; expecting, I suppose, that these bodies would take the matter up, and put me on my defence. The Council of the Astronomical Society, to whom Sir James and I were both well known, and who had also had, not long before, a striking proof that the *Mechanics' Magazine* is not precisely a trustworthy authority, slighted the affair. I can speak less confidently of the Council of the Royal, but I know that no explanation was asked of me.

Baffled in these attempts, Mr. Babbage brought forward the *substance* of Sir James's letter, as a charge against me at the meeting of the Board of Greenwich Visitors in June 1853, and I then learned, for the first time, what I was accused of. My reply was necessarily short and uncircumstantial ; but while I admitted

that I had, thirty years ago, introduced a foreign instrument without payment of duty, I denied, in the flattest and least civil language, the *truth* of the rest of Sir James's story ; and I believe that the gentlemen who heard me had no doubt of my veracity.

At the last Anniversary Meeting of the Royal Society, Mr. Babbage was allowed to read the letter from the *Mechanics' Magazine*, and to ask for an explanation ! This I proceeded to give, but was stopped just as I was proceeding to show the incompatibility of Sir James's *present* story with the *whole* of his *previous* conduct.

In the present letter I have shown, I think, circumstantially and conclusively, the patent falsehood of Sir James South's reminiscences. I have entered into more detail than is necessary for those who know both parties, but it is my duty to the gentlemen whom I am proud to call my friends, to justify their confidence in me as completely as lies within my power. If a libeller puts his libel into the mouth of a dead man, and asserts that the conversation took place between the two, I see no possibility of doing more than I have done. I deny the *truth* of the story, I show that the witness is *unworthy*, and I point out that he is *contradicted* by his own *subsequent* conduct, and by the *whole* life of his *supposed informant*. I beg of any one, who has still any doubt left, to inquire from those who know Sir James and me, what is our relative authority.

I attribute Mr. Babbage's blundering pertinacity to a diseased mind, and I believe this conclusion is far from being confined to myself. I should be glad to make the same excuse for Sir James South ; but I must frankly own that his conduct is *not* repugnant to his character, and though he has shown more boldness than I gave him credit for, I attribute this rather to a defect in apprehension than to a superfluity of courage. The *Mechanics' Magazine* is labouring in its vocation, and suiting its wares to its customers ; and yet I think this journal, too, had its own inducement.

I have thought it best, while I was engaged with Mr. Babbage as the instigator, bottle-holder, advertiser, and placard-bearer of Sir James, to settle our own private dispute. For the honour of Cambridge (though I scarcely allow him to be *pur sang*), I hope he will state more truly and argue more logically in his reply, if he makes one, than he has done in his assault.

I regret that my justification has required me to quote conversations with persons now deceased. It must be remembered that the publication of a conversation *supposed* to have been held with the late Mr. Troughton, is the cause of my troubling the public at all.

R. SHEEPSHANKS.

A LETTER, &c.

THE members of the Board of Greenwich Visitors, who were present at the meeting in 1853, will remember that Mr. Babbage brought certain charges against me, founded on a letter by Sir James South, published in the *Mechanics' Magazine*. It will also be remembered that I gave the most positive denial, and in the least courteous terms, to the graver part of those charges, declaring them to be the simple invention of Sir James South and without any foundation. I do not complain, but the Board surely may, that no notice was given either to me or to them of this intended attack, and that no copy of Sir James South's letter was produced: this want of business-like conduct on the part of Mr. Babbage wasted a great deal of our time and rendered the discussion less specific and satisfactory than it might have been, had I previously known the nature* of the charges. I do not think any of the gentlemen present doubted my statements, and even Mr. Babbage himself, if capable of reflection, must have seen that his prejudices had warped his judgment.

At the subsequent Anniversary Meeting of the Royal Society, however, Mr. Babbage renewed his assault, and now read the letter of Sir James South, requesting, if I remember correctly, that the President would call upon me for an explanation. This, after some interruption and confusion, I proceeded to give; but as the language I happened to use, though appropriate and parliamentary, was not sufficiently honied to suit the tastes of several gentlemen present, who had heard me charged with an attempt to suborn perjury without being at all shocked, I was stopped in my defence just as I was entering on the most essential part, which would scarcely have occupied three minutes.

The Bye-laws of the Royal Society direct that any charge against a Fellow of the Royal Society shall be brought before the Council, and Mr. Babbage had already attempted this course with the Councils of the Royal and the Royal Astronomical Societies, but without success. He might, *perhaps*, have legally moved a vote of censure on the Council of the Royal Society for neglecting his accusation: his actual proceeding was entirely irregular and contrary to law as well as to good manners. (*See Appendix*, No. I.)

* I did know that Sir James South had attacked me in the *Mechanics' Magazine*, but I had not seen the letter nor had any idea of its contents (further than that it related to my introducing an instrument without payment of duty), until Mr. Babbage enlightened me.

I am not going to make a great grief of this unjust and ungentlemanlike behaviour of some members of the Royal Society (for harsh as the term may seem, a denial of justice is ungentlemanlike), but I must say that I feel nearly as much ashamed for them as I am indignant for myself. A gross breach of the law was permitted, a grave accusation was made, and the answer, which would have shown the absurdity of the accusation, was cut short by misplaced and cowardly delicacy. I was told, indeed, by several gentlemen, that any further defence was unnecessary, that I had sufficiently exposed the improbability of Sir J. South's assertions, and that I might be quite satisfied no one believed him. Supposing all this to be true (and yet we know how often, when dirt is boldly thrown, a little will stick), I cannot help feeling that I did not deserve the treatment I received; and I must express my belief that an ordinary meeting of mechanics would not have allowed an inoffensive member to be thus attacked, *in clear breach of their own regulations*, on any such trumpery grounds at all: still less would they have interrupted his defence.

I have no intention, however, of establishing a feud with the Royal Society, or any part of it; and having protested against their conduct towards myself, and against the precedent, I will proceed to state the charges brought against me by Messrs. Babbage and South, with my explanation and defence.

Sir James South's letter was published in the *Mechanics' Magazine*, January 24, 1852, and is as follows:—

“IN RE BABBAGE V. SHEEPSHANKS.

“‘If this be not subornation of perjury, it is very like it.’—*Mech. Mag.*, Jan. 17, 1852.

“Sir,—The perusal of the able article in your Journal, from which the above extract is taken, has called to my mind a parallel instance of quasi-subornation of perjury, which you may, perhaps, deem not unworthy a corner in your pages, illustrating as it does very strongly, how British workmen are but too often injured in their reputation by foreign counterfeits, and how the practice derives encouragement from the low state of moral feeling prevailing as well among scientific (or rather pseudo-scientific) as among fashionable circles.

“For very many years I was on terms of the closest intimacy with the late Mr. Troughton. Calling, as was my habit, almost every day, I found him on one occasion in a state of great agitation. I asked him ‘What was the matter?’ He said, ‘That fellow, Dick —, has just left; he has been abroad, and has brought from Paris one of Jecker's circles: he tells me, “that to avoid payment of duty for it, he has had the name of ‘Troughton’ engraved on it;” and he has asked me “to let one of my workmen go down to the Custom-house, and clear it for him as an *English* instrument.” I told him I would rather cut off my right hand than be concerned in such a rascally transaction: and from what

he said, I am not sure if W—— is not as deep in the mud as Dick is in the mire.' I replied, 'I hope not.' Mr. Troughton then said, 'I told the fellow, if he wanted to rob the Revenue by perjury, he must get some other person to help him; and he went away in great dudgeon.'

"Some few days afterwards, calling on my old friend Troughton, I crossed him in the passage, between his shop and his parlour, as he was coming down-stairs. Taking me by the hand, he led me to the window at the further part of the room, and bowing to the window-sill, he introduced me, with a look of contempt which I shall never forget, to a *circle* which was lying there. He put it into my hands, saying, 'It was the Jecker's Circle which S—— had got from the Custom-house, but whether by swearing to a lie himself, or by having gotten some one to swear to a lie for him, he did not know.' He pointed to the name of 'Troughton' engraved on it, and said, 'The imitation was a very good one, and the fellow was an expert forger.'

"I am, Sir, yours, &c.

"JAMES SOUTH.

"Observatory, Kensington, July 19, 1852."

The shallow cunning with which Sir James attempts to avoid the responsibility of his libel would not have saved him if it had been worth my while to prosecute; but the attempt is a sample of the happy manner in which his spite is tempered by cowardice and folly.

There is, generally speaking, this difference between a true story and a false one, that the first can bear a close sifting and examination, while the latter, unless it is the work of a master, can not. Sir James South is no Defoe, nor has he the proverbially necessary gift of a long memory.

An insinuation is clearly made in the first paragraph that I wanted to affix a fraudulent value to a second-rate instrument, by attaching Troughton's name to it. How, then, did the instrument come into Troughton's hands? Surely he was the very last person to whom I should have sent it, if I had wished to pass it under his name, and to the "injury of his reputation." I may also ask, in what way could I merit the designation of "an expert forger?" I could not engrave the spurious inscription, for I was in England all the time; but if I had been in Paris, how could I have taught a French engraver to write like an English one? Troughton's name was (I think) rightly spelt—rather a remarkable thing—but the writing was altogether and unmistakeably French.

That the language attributed to Troughton is "arrant South," and quite different from his own genuine Anglo-Saxon, is no solid objection to the *general* truth of the story. Sir James could no more copy, or even recite, the *ipsissima verba* of his "revered friend," than Dr. Johnson could make little fishes talk any language but that of big whales. Sir James, professing to quote "the very

words" of his authorities, evidently uses his own. It is desirable that this peculiarity of "ornamenting everything he touches" (and I wish his license extended no farther) should not be overlooked.

I will now state the real circumstances of the case, and in more detail than I could well do, *vivâ voce*, at the meeting of the Visitors (when I was so unexpectedly called upon), or at the Royal Society, where my condensed defence was so irregularly stopped.

It is known to several members of the Board that I have in my time paid considerable attention to astronomical instruments — and, indeed, was even a sort of authority — until my excellent friend, the Astronomer Royal, took the wind out of my sails. I was particularly smitten with the principle of repetition; and after having purchased a Borda's circle by Troughton (which introduced me to his acquaintance), I was most anxious to obtain a Borda's *reflecting* circle by him. I soon discovered that from Mr. Troughton nothing could be got, and least of all an instrument the principle of which he disliked. In this dilemma, I saw a favourable account, by Sir Thomas Brisbane, of the performance of a circle of reflexion by Jecker of Paris; and, as a friend was going to Paris, in the winter, I believe, of 1823, I requested him to procure me a circle of Jecker's and to get Troughton's name engraved upon it, so as to pass our Customs without duty, and without causing him delay or trouble. This was done, and the instrument, I am pretty sure, left by him at Troughton's shop in Fleet Street, to have the inscription erased, and to be adapted to the stand of a British circle which Troughton had recently sold me. It is most probable that the officers at Dover had their attention drawn to the erroneous inscription by the commissioner, and so passed it; but I can say nothing about this of my own knowledge.

I own that I am now heartily ashamed of this transaction, although everybody smuggled in those days, directly or indirectly.* The absurdity and injustice of our fiscal laws were self-evident, and, consequently, few felt bound to obey them. To me, then a student in the Temple (being, as I was already, a freetrader of the first water, and not favourably disposed towards the financial arrangements of an *unreformed* house), the duty on astronomical instruments was particularly ridiculous, the article being as little likely to be imported into England from France, in those days, as coals into Newcastle. I should be surprised if the duties on astronomical instruments imported from France into England in ten years from the peace, amounted to as many shillings. The duty, like some others, must have been imposed merely to complete the symmetry of the stupid system of *protection*, now happily disappearing; and had not the poor merit of the laws compelling burials in woollen and the use of metal buttons: there were no imports on which it could be levied.

I had, besides, my own grievances against the Customs, as I sup-

* At that time Bandana handkerchiefs were *contraband*; yet every gentleman, as Mr. Hume remarked in the House, had a bandana in his pocket, from Mr. Speaker downwards.

pose most persons had who travelled thirty years ago. From a mistake in form, I had some time before paid the duty upon a Geneva watch *according to the price paid at Geneva*, and not, as the law is, *on the value assigned*; a difference which, in this case, amounted to several pounds. Another time, returning through Brighton, my party and myself were detained several hours in a passage of the Custom-house, while a select portion of the Brighton unwashed was gathered round the doorway; and this because the officer had no competent acquaintance with his duty,* and could not get through his work.

The law being ridiculous and ill-administered and enacted by an unreformed parliament, I leaned, I fear, to the doctrines of my Cambridge fellow-student, "Alein, the clerk":—

"For, John, ther is a lawe that saieith thus,
That if a man in o point be agreved,
That in another he shal be releved."

I looked, perhaps, upon smuggling as a sort of "reprisals" on an enemy. Whether the idea of putting a false mark to mislead the officers was suggested by my Brighton experience of their stupidity, or by a common practice of traders, or by a wish to triumph over the ignorance of the searchers (the deception was so gross as to give me that sort of satisfaction), I cannot now recollect; I rather think that a wish to spare Mr. — any delay or trouble in paying the duty was my real motive. The duty itself, on a proper estimation of the instrument, could not have exceeded fifty shillings or three pounds.

I shall scarcely be held to justify this transaction now; as I said before, I am heartily sorry for it. I admit that silly and oppressive laws must be obeyed so long as they continue to be laws; and though, in point of fact, the evasion of such laws has generally been the efficient cause of their removal, I allow that the only proper mode of proceeding is to press earnestly and peaceably for their abolition, obeying them meanwhile. It is not enough to say that the maker of the bad law is more to blame than the breaker. *That is true*; yet the general principle of obedience to law is one too valuable to be broken, in any case, unless where a positively wrong act is commanded to be done. Happily our reforms in the last thirty years have tolerably reconciled our laws with common sense and common fairness; but I ask, and have a right to ask, to be judged by the ordinary practice thirty years ago, when the occurrence took place.

* The officer, who had been a clerk in the Customs, suggested to the Board that the clerks were more numerous than necessary. On inquiry this was found to be true, and the reduction extended to himself; as a compensation, he was sent to Brighton as a searcher. This I learnt afterwards from Commissioner R—. So ignorant was this person of his duty, that I was forced to ask him for the tariff, and to teach him the difference between things contraband and those paying duty. He would have charged, but for my interference, a poor French mechanic for three or four books printed in England. Some trifling things were stolen from us on this occasion by the lower officials, but no redress could be had.

After this confession I may be allowed to say how restitution was made. Some years later, I imported a theodolite from Munich, and applied to the Treasury to have it free of duty. The reply not coming as early as I expected, and the Customs officers assuring me that leave would not be granted, I paid the duty. A few days afterwards I received permission to bring in my circle duty free, and applied to the Custom-house for repayment; but the refunding what had been once received, was so repugnant to the usages of the establishment, that I gave it up. As the Munich circle was worth the Jecker's circle at least thrice over, I consider the Customs to have been no losers by me.

I have already mentioned that the instrument was delivered *directly* by Mr. — to Troughton, and I am as certain of this as I can be of anything which I don't positively recollect. I am *sure* it did not pass previously through my hands. I remember, most distinctly, that I learned from Troughton's own mouth, how Jecker had executed my commission of engraving his name. There was no need of erasure, he said, for Jecker had engraved his own name and then screwed over it a small plate with his (Troughton's) name. He said, too, that the work was better than the French work he had formerly seen (I am pretty sure we had not then heard of Gambey, and that Troughton alluded to Fortin's circles), but that he did not like well enough to adopt it as his own.*

Now this is all that passed between Troughton and myself on this matter, I am positive; and as I am sure he never said knowingly anything that was untrue, I assert that the additions with which Sir James has garnished my simple story are the coinage of his own "base and bitter" imagination. I had nothing to do *personally* with the introduction of the instrument. I never applied to Troughton to procure me false evidence (he was certainly one of the last men to apply to in such a case) nor to any one else, nor did I take any step to procure the admission of the instrument, either by myself or through any other person, beyond what I have already stated. I give the most flat and positive contradiction to Sir James's "recollections;" and if I do not repeat the still more offensive word which escaped from me when I first heard from Mr. Babbage what I am charged with, it is because I don't know the mental state of Sir James at the time he *published* this story. He might—

"Like one
Who having, unto truth, by telling of it,
Made such a sinner of his memory
To credit his own lie,"—

* As an illustration of the proverb "Cheating never thrives," I may mention that this unlucky circle was immediately seen to be unfit for the delicate purposes for which I designed it, and I am not sure that I ever made an observation with it. Some years afterwards, it was pulled to pieces in order to use the limb for a heliostat; and I believe that, for the last twenty years, if in existence at all, it has formed a part of the lumber of Mr. Simms's factory.

really have *believed* in 1852 what he had no suspicion of in 1824, nor some years later.

To gentlemen who know me and Sir James South, I, perhaps, need not say any more; he asserts, and I deny,—*utri creditis Romani*. But I will propose to Sir James a few inconsistencies, and I will trouble him or his ally, Mr. Babbage, to clear them up.

He says, and I suppose Mr. Babbage believes him (we are all agreed, I fancy, to grant implicit credit to Troughton), that I proposed to Troughton to lend me a man to declare the circle of British origin, which Troughton most indignantly refused.* He also says that Troughton, “a few days afterwards,” repeated this fit of virtuous indignation; but now, *in the presence of the offensive circle, which was quietly occupying a place in Troughton’s shop*. Though Sir James does not tell his story very clearly, the meaning must be, that I applied to Troughton (the circle being in limbo) to assist me to liberate it by a false oath, that he most indignantly refused, that I then got it out myself by making a false declaration, personally or by proxy, and then sent it to Troughton *as if nothing disagreeable had happened*; that *he took it in with the same nonchalance*, reserving to himself the privilege of *venting his indignation in big words* to Sir James South. It is now many years since Troughton died, but some of the gentlemen I am addressing may remember enough of him to judge whether he could have acted thus inconsistently and weakly. I ask, too, whether such an imputation on Troughton can be rendered probable by such a witness as Sir James South, twenty-eight years afterwards?

In addition to these inconsistencies, I think people who know me will not find any compatibility between the language said to have been used to me and my subsequent conduct. I certainly never was addressed in any such terms as those which, according to Sir James, were applied to me by Troughton; but I think, though not very implacable, I should have given a very wide berth indeed, for the future, to the man who had used such language to me.

An account of the subsequent relations between Troughton and myself will, I think, throw still further discredit on Sir James South’s late recollections, and show that he has put words into his “old friend’s” mouth, which his old friend would no more have uttered “than he would have bitten off his tongue,” if I may be allowed to adopt Sir James’s style.

I am pretty positive, from a number of circumstances, that Jecker’s circle was introduced into this country early in 1824. I suppose, therefore, the date of the reported conversations was about February that year, not long before Sir James South took up his temporary residence in France. One of Sir James’s

* I go further: my vigorous and high-spirited old friend would have kicked any one out of his shop who had made such a dishonourable and insulting proposal; and would have repeated the process, *toties quoties*, if the scoundrel who made it had shown himself there again.

favourite projects was to induce his "revered friend"—his "almost second parent"—to visit him in the summer of the same year, only a few months after the imaginary conversations. Is it not somewhat strange—or, rather, would it not be strange, if Sir James's story were true—that *Troughton would only consent to pay this visit, on condition I would accompany him and take him in charge?* He was then about seventy, rather infirm, very deaf, and not speaking or understanding a word of French. I am *certain* that Sir James South himself very strongly pressed me to undertake this journey, and I am almost certain that I was invited to take up my abode with him at Passy. Troughton and I did accordingly journey together most amicably to Paris and back again; and so satisfied was he with his companion, that, on a later occasion, he refused to go to Oxford, unless I would again escort him, which I did.

As Troughton and I could not see St. Denis on our return, on account of the funeral ceremonies of Louis XVIII.—as I remember, moreover, my disgust at being compelled to get a black suit for the general mourning (Sir James kindly recommending the tailor)—I can date our visit very nearly; it must have been about the end of September 1824.*

After our return from Paris (Troughton smuggled three or four bandanas for me, which, as he took snuff, was a convenience to both), Troughton got my consent to propose me for the Astronomical Society. I was proposed November 12, 1824, elected January 14, 1825, and immediately placed on the Council, February 11, 1825, most probably on his representation. I have been asked if Sir James signed my certificate; he did not; he was then residing at Passy.† It is, perhaps, a better mark of the value Troughton set upon me that he introduced me to his very dear friend, the late Astronomer Royal, Mr. Pond, whose confidence I may say I possessed, and tried to deserve.

That Troughton's regard for me did, in some respects, go

* I will here add a trifling circumstance, which, to some minds, may have a little weight. Though Sir James was in England shortly before our visit, he was unreasonable enough to ask Troughton to carry over a pointer, instead of taking it himself. I should have declined such a troublesome commission; but Troughton had accepted it, and the dog was in his keeping before I knew anything of the matter. As he assured me the dog knew him, I gave an unwilling assent, and Dido with her basket was strapped on our carriage. But the dog neither knew him, nor anything, nor anybody else, and gave me infinite trouble and anxiety on the road. Now as Sir James must have known the unfitness of Troughton to take care even of himself, and, therefore, that the dog must fall entirely to my charge, is the liberty he took with me in harmony with the conversations which are supposed to have occurred only a few months before?

† I have been asked whether Sir James South signed my certificate as a candidate for the Royal Society. He did not, neither did Baily or Pond (both of whom were members of the Council that year), or Troughton. It is clear I *could have asked no one*, as these names are omitted. I should never have applied to Sir James *in any case*, but at the date of my certificate, Jan. 14, 1830, he had been (as President of the Astronomical) under my guardianship for nearly a year. He was, moreover, at this time in bad odour with the Royal Society, though the *Thirty-nine Charges* were not printed till some months later.

beyond my merits, I freely allow. He once told me that he had known only three people who handled an instrument to his liking — Pond, Kater, and myself. Now this praise was, undoubtedly, rather an expression of liking than strictly just. But I listened to him attentively, and understood and discussed his various constructions with interest, and so, perhaps, passed for a more handy person than I really was.

My intimacy with Troughton suffered no interruption from my coolness with Sir James South. The quarrel between Troughton and Sir James, with respect to the great equatoreal, was thoroughly established, I think, in the latter end of 1832. Calling one day in Fleet Street, I found the old man somewhat discomposed; and he then (after remarking that I was, probably, surprised that he and Sir James had continued friends so long) placed in my hands some letters, from which I concluded that the quarrel was not to be made up.

After some consideration, I now thought it my duty to interfere. Troughton was too old and infirm to do anything effectual in the business: Mr. Simms, his partner, was too much engaged, and was, besides, too sensitive to deal with such a rude customer. It was clear no light would come from the science of Campden Hill; and I felt that, without some assistance, my old friend Troughton, who had been almost forced into this undertaking by the importunities of Sir James, and my younger friend, Mr. Simms, who had merely executed what Troughton and Sir James directed, would be "done" out of their money, worried to death by impertinent letters, and probably professionally injured by the scandal of Sir James and his allies and toadies. I had some confidence in my own skill, but my chief reliance was on Mr. Airy; and my hope was, that a person of fair intelligence, with abundant leisure, would be able to extract help from different quarters, and to combine the whole to purpose. And this I succeeded in doing, as far as Sir James would permit; at last he closed his doors, refusing to pay or to allow the instrument to be put in working order, *though we undertook that this should be done at our expense.*

I know that my services in this long and troublesome business were duly valued by both Troughton and his partner. When I showed the instrument in a merely temporary state to Troughton, he said it was very nearly as good as he ever expected it to be, though the bracing consisted merely of a few pieces of quartering, and the clock was of the rudest description. The only remark which he made during the contest was, that I had not acted with sufficient vigour towards Sir James. "You should have arrested him," he said; "the fellow has a white feather. Frasi arrested him, and got paid." I suppose I shall not be greatly blamed for preventing the arrest of Sir James, considering the terms on which Troughton and he had long been; besides, whatever may be the colour of Sir James's feather, I did not see how forcing him to

get bail would advance matters. It would have been an insult, nothing more.

Some objections have been taken to my interfering at all, and also to the manner of my interference, but I think unfairly. It is the opinion of all persons conversant with the parties, that Sir James would not have paid except on compulsion; and that no one except myself could have been found to carry this compulsory process into execution. I myself have no doubt of this. The verdict pronounced by Mr. (since Mr. Justice) Maule,* who was arbitrator in the case, gave us every shilling that we claimed, thus declaring the justice of our demand. One portion of the expense, amounting, if I remember rightly, to about 200*l.*, which was for additions and alterations directed by me, was, by agreement, *only to be paid if effectual*. Now the arbitrator had the instrument exhibited to him for some hours on a fine night, and therefore was able in a considerable measure to form his own opinion independent of the evidence. The verdict, therefore, establishes the utility and efficacy of my alterations, — *they were paid for*.

Before commencing my examination of the instrument, I stipulated that I should not, when at work, be interrupted by Sir James South. I knew he was too ignorant to be of any use, had he been disposed to be so and I disposed to consult him. I felt, too, that his strutting about the room, whistling at the same time most untunably, would not aid me in the working out a difficult problem. Besides these very sufficient reasons, I wished to prevent silly letters to the "*Times*" which he would certainly have sent *à propos* of everything he either did not understand or misunderstood. This necessary precaution he chose to interpret as a barring him out of his own observatory; and I have been told that, when he opened the door of the dome for visitors, he kept himself ostentatiously back. Indeed he made this supposed condition the subject of an attack in the "*Times*" newspaper, in which he alleged that he had been excluded from his observatory by "ecclesiastical malevolence." This absurd phrase was, of course, unintelligible; and Sir James was obliged, in a second letter, to name me as the "malevolence." I rather think he thought it necessary to assure the Bishops, *seriatim*, that the blow was not meant for them, and I could, perhaps, have got hold of one of these curious circulars; but I avoided the temptation of being induced to prosecute a man for whom I felt myself more than a match at his own weapon.

* Perhaps the most important service I rendered to Messrs. Troughton and Simms was by insisting that Mr. Maule should be the arbitrator, if the cause were withdrawn from a jury. I had no personal acquaintance with this gentleman, but I knew that he was the crack senior wrangler of his time, and in the opinion of many persons, the man most likely to have been the Laplace of England if he had devoted himself to pure science. The readiness with which the arbitrator caught all the mechanical points, though the subject was perfectly new to him, and the quick manner in which he detected an erroneous conclusion which had escaped us, and thus furnished the explanation of an important experiment, convinced me that his Trinity reputation was not exaggerated.

In the correspondence which followed this outbreak, I have no reason to think that Sir James had the advantage, *and I got the last word.*

I have been accused of keeping up the irritation in Troughton's mind against Sir James; and Mr. Babbage and Sir James South probably think so, as it is just what they would have done in my place. But the suspicion is altogether untrue. I had no need to exasperate Troughton, and when he went beyond what I thought was true in his diatribes, I expressed my dissent. Upon one occasion he called Sir James a "deep, designing knave; who had shown great anxiety about his health, but he now believed merely for a selfish end." I replied, that I did not think so; that I believed Sir James once had a sincere regard for him. I own that when Troughton called Sir James "a dirty rascal," I offered no opposition.

Troughton died in 1835, before the trial was decided, and left me an executor of his will, with Mr. Simms and a relative. Mr. Simms and I had the pleasure of putting an end to a foolish misunderstanding among the legatees, which might have easily led to a Chancery suit, had we not allayed their temper, and that of our co-executor. Finally, I employed the larger part of a small legacy, which Troughton left me, in a monument to his memory; and though I made no professions of "veneration," and never adopted him as "a father," nor made after-dinner speeches in his honour at seasonable and unseasonable times, I am satisfied I was a friend far more to be desired than Sir James South.

I fancy that I have said quite enough already, to convince every person who knew Troughton, that the conversations reported in the *Mechanics' Magazine* are the coinage of Sir James South's brain. I shall now try to show that this calumny is comparatively of recent origin, and that Sir James *himself* did not believe it for at least ten, perhaps twenty, years after my unlucky circle had been honoured, *as he says*, by Troughton's salute.

My acquaintance with Mr. South, for I have had very little to say to Sir James, must have commenced before his visit to France, for I remember that I was several times at his house in the Borough, where I was always most hospitably welcomed. Probably we became acquainted at Troughton's, on whom each of us frequently called. I conceive that this acquaintance must have commenced in 1823, but I cannot recollect anything to show that I visited in Blackman Street after the scene in Fleet Street so graphically described by Sir James,* though I have no doubt that I did. But after that event—and not long after—as I have already said, he urged me very pressinglly to take charge of Troughton on a visit to him at Passy, and, I believe, to take up my abode in his house; which I declined. As I was a frequent guest at Passy,

* I have a dim recollection that Sir James did not return to the Borough after he had lived in Paris, but I cannot at present remember anything which would give me a date. I never visited him in Sloane Street.

and as Sir James saw no impropriety in introducing me to all his guests, consisting of the *élite* of the *savans* of Paris, he could scarcely *at that time* have believed the story which he has since made public. It may, perhaps, be alleged that, in his desire to compliment Troughton, he put a force on his feelings: those who like to think so may, but those who best know him scarcely will.

On Sir James's return to England, though our acquaintance continued, and we were familiar when we met, I don't think I visited him often, though I am pretty sure I have dined with him at Campden Hill, and quite certain that I have been invited and declined. Indeed, I began gradually to get ashamed of his turbulent and quarrelsome behaviour to other persons (to myself he never was otherwise than respectful and attentive), and to doubt whether I had a right to keep neutral. In the trial about the equatoreal, Sir James spoke of me as one "whom he once thought his friend," but this was not correct. I was an acquaintance, and a tolerably familiar acquaintance, but I eschewed his confidence and never gave him mine. In February, 1829, Sir James was elected President of the Astronomical Society, chiefly, I think, on the pressing recommendation of three members, one of whom, Lieutenant Stratford, then urged my appointment as Secretary, *to keep the President in order*. This disagreeable office I endeavoured to execute, and I believe with some effect; but assuredly this strange official relation caused some unpleasant feeling on both sides, though I am bound in candour to admit that Sir James bore my tutelage, on the whole, very tolerably.* The business,

* Sir James South found out very early in our acquaintance that if he did not mean to realise the fate of the earthen pot, he must not jar too rudely on me. He knew nothing, and was well aware that he knew nothing; and he knew, besides, that I had no difficulty and not much scruple in setting him right when he got out of his depth, which was very often. Sir James does not know a sine from a cosine, and is not able to use a table of logarithms for the simplest computation. The solution of a plane triangle is quite out of his reach. His acquaintance with the principles of astronomical instruments only extends to some half-dozen precepts of Troughton's; which he practises, probably, without understanding. I do not believe he could ascertain the errors of adjustment in a transit; he would have to correct the errors by actually altering the screws, and by successive bisection of the apparent errors. Troughton's account was, that he attempted the "Asses' bridge," but could not cross it. Fallows said (so I was told by Dr. Pearson) that he never could teach him the difference between + and - : but this I hold to be only figurative. I heard Sir James swear that he could solve, or had solved, a quadratic equation,—*sed non ego credulus*; perhaps he learned the rule by heart. His mechanical knowledge used to be pretty much on a par with his mathematics. He had a good eye, nice hand, great boldness, and at one time much energy and industry; and as he bought excellent instruments, and worked under able direction, his observations have considerable value, though they are not such as would be deemed first-rate now. But Sir James South's reputation rests chiefly on another foundation. He gave many dinners, boasted himself before ignorant people, abused his betters in the newspapers and in pamphlets, published the works of others as his own, and physicked the wives and children of his acquaintance. If I am provoked, and think it worth while, I may some day execute an analysis of Sir James South's publications, giving his due to each contributor, and extending my list of *Southiana*.

however, was so irksome to me, that in February, 1831, I quitted the Council, assigning openly as my reason that I was too busy to attend, but telling my particular friends that I had belled the cat long enough, and was tired of such hangman's work. Immediately afterwards I was engaged in an open quarrel with Sir James; and as the circumstances were only incompletely known at the time, and have some interest in the history of the Astronomical Society, I will relate them here as briefly as I can.

In the second year of Sir James's Presidency a wish was expressed by several members of our body that the Society should be incorporated, and after some discussion it was agreed that a petition should be presented for that purpose. There was to be only one petitioner, to save fees; and as Sir James was President, it was agreed that he should be the petitioner.*

From delays, *caused by the President alone*, this Charter, which the Society had agreed to in or before June 1830, was not signed till March 7, 1831; many months after the time expected; and after Sir James South's Presidency had terminated, which it did in the preceding month. Now the Charter contained the following clause: "That the members of the Council shall be elected within six calendar months after the date of this new Charter; and that the said Sir James South shall be the first President." This clause, which was inserted in full confidence that the Charter would be signed before November (as it might have well been, but for the *laches* of the President), became a stumbling-block when the Bishop of Cloyne had been duly elected in February 1831, and actually filled the chair.

Everybody would suppose there could be no real difficulty; that Sir James South, who had been honoured so far above his merits

* With one or two others, I objected in the Council to a Charter; but gave way, partly convinced, to the majority. At the evening meeting, Sir James, as President, communicated the matter to the Society, but in such bad taste and with so little motive, that some of the members—the late Captain Basil Hall was one—asked for an explanation why a Charter was to be desired. The President's reply was of the dignified kind,—“that he did not sit there to answer questions,” which seemed neither civil nor satisfactory. So I came, good-naturedly I think, to his rescue, and produced reasons which, whether good or bad, prevented any breach of harmony.

I must leave this expression of self-compliment, by admitting a gross breach of duty on this occasion, from a foolish feeling of false delicacy. I ought not to have allowed Sir James to be the petitioner, and it would have been quite sufficient to have named Francis Baily; there would have been no opposition. Dr. Pearson first suggested an Astronomical Society, in 1812 I believe, but the time was not then fitting. Mr. Baily took up the matter several years later, after the Geological Society had prepared the way; and he is the *real* founder of the existing Society. Sir James South was an early, but not, so far as I can make out, a conspicuous member. After Sir John Herschel, Mr. Babbage appears to have been Mr. Baily's most active supporter; while Dr. Pearson and Troughton were very effective coadjutors and canvassers, but rather on their own hook and in their own way. The Charter of the Royal Astronomical Society, which says that Sir James South, “together with others of our loyal subjects, did in the year 1820 form themselves into a Society,” must be understood as I have above narrated. It is not false, but might give rise to an erroneous interpretation.

as to be elected President, and to have been appointed sole petitioner (passing over Baily, who really had formed the Society), and who, moreover, by his own carelessness, created the hitch,—everybody, I repeat, would suppose that Sir James, of course, put himself immediately into the hands of the Council, and offered his utmost exertions to set everything straight. But he did nothing of the kind; he kept out of the way, made no arrangement, and said, in his swaggering way, that the Society was in a confounded scrape, and that if he chose, he need not call a Council for six months.* All this I heard from Troughton; and, though I was no longer a member of the Council, I went immediately to Lieutenant Stratford to inquire into the state of affairs. I think this must have been on the 11th of March. From him I learned that the Charter had just been signed, and was in his possession, but that Sir James should not have it till he came to some reasonable terms. I explained to Stratford that the Charter belonged to Sir James, and must be sent forthwith, assuring him that it could be of no effect until we—the Society—accepted it. There was, I believe, rather a stormy meeting at the Council on March 11, where Sir James went after he had received the Charter; but it was finally agreed that the Charter should be accepted by calling Sir James to the chair, and that the meeting should immediately proceed to re-elect the Bishop of Cloyne as President and the existing Council: also that the immediate consideration of the bye-laws, which Sir James objected to, should be postponed, although notice had been given that they would be proposed.

At the evening meeting, the Secretary, Mr. De Morgan, stated the course of proceeding which had been agreed upon, when Sir James, in violation, as I have always understood, of his express engagement (supposing that he was in his sober senses at the time, and was cognisant of what he had promised), rose up and made a rambling speech, thanking God that he was a man of honour, and much to the like purpose. The meeting, which was wholly ignorant of the underplot, wished to proceed to accept the Charter; while those who knew Sir James better, objected to destroy the old Society before they had provided for its resuscitation and re-organisation. I do not to this hour know what object Sir James had in view when he made this speech, and, perhaps, he knew as little; for when I asked him to say what he wanted, he replied by stating that the meeting was not legal, *for the notices had been sent out a day too late*;† a fit and appropriate return for Mr. Stratford's thoughtless delicacy. This objection stayed the whole proceedings; and the breach of faith so much irritated the Council, that at its next meeting, March 19

* It is not very safe to impute motives, but the only motive I can assign for Sir James's behaviour is, that he expected the Council, to escape the difficulty, would offer him a second term of office for six months, or, perhaps, longer. I leave this explanation to him.

† The notices *were signed* in due time, but Lieutenant Stratford delayed posting the letters for twenty-four hours, that he might call on Sir James and explain and conciliate matters.

(the last Sir James ever attended), he found that he had gone too far. So fresh notices were issued, and on the 6th of April following, Sir James being proclaimed three times, and not appearing, Mr. Bryan Donkin was called to the chair, the bye-laws were passed, and the existing President and Council re-elected, with some exceptions.

I had been so much annoyed by Sir James's extraordinary conduct, and the trouble he had given the Society, that I told Lieut. Stratford I should object to Sir James's re-election as a member of the Council. I said I should bring forward my objections under two heads: 1st, that from his ignorance he could not give good counsel; and 2ndly, that from his infirmity of mind he could not keep good counsel. After some discussion, however, and on a representation that such an onslaught on the late President might injure the Society and annoy some persons whom I respected, I said I would be satisfied without any direct exposure, if Sir James's name were withdrawn from the Council. This was accordingly done; and as my objection to serve no longer existed, I consented to occupy his vacant place.*

Sir James gave the R. A. Society no symptom of his existence till the General Meeting of February 14, 1834, when he came down, and, in one of his usual speeches,—

“A tale

Told by an idiot, full of sound and fury,
Signifying nothing.”†

proposed that the gold medal should be given to Lieut. Stratford, the Editor of the *Nautical Almanac*, for the production of that work. The Council, in which the Superintendent had many very warm friends, would most gladly have given him their medal, if it could have been done without breaking all their usual rules; so the Secretary quietly answered by pointing out the objections to such an appointment, and by calling Sir James's notice to the fact, that the bye-laws, about which he had formerly made such a fuss, placed the distribution of the medal *exclusively* in the hands of the Council. I followed on the same side, when Sir James accepted to my remarks as personal and out of order. The Chairman overruled him, and I was proceeding with my task of castigation, when Sir James could bear it no longer, and hobbled out of the room; and so it is only with some allowance I can say with the orator, *Abiit, evasit, erupit*. As it was necessary to stop such nuisances, I moved a vote of censure, in the shape of an amendment, which was carried *nem. con.*, Sir James's seconder holding up his hand with the rest.

* See the Appendix No. II., in which I have given the minutes of Council and of the Special General Meetings.

† Sir James's oratory and delivery is of the purest British lion stamp—a tissue of claptraps and nonsense, most energetically pronounced, and with a remarkable emphasis on the letter *r*. But you have only to look at him and contradict him, and he is a lost lion.

The first effect of this signal defeat was, I believe, rather stunning; but in a few days, Sir James, who, like his brother-knight, Sir Andrew Aguecheek, if he redeems opinion in any way, conceives "it must be with valour," hit upon the bright idea that he might insult the mover and seconder of the amendment. I, the mover, might be safely disregarded; and Mr. Frend, whom he took erroneously to be the seconder, was so old and so prudent a man, that he, too, was not dangerous. He threatened Mr. Frend, therefore, "that he would expose him, though with great regret" (I think the exposition was to be to all Europe), "for the share he had taken in the affair." Luckily for Sir James, before he committed any overt act, he learned that my seconder was Captain, now Admiral Smyth;* and the exposition was no more heard of.

Since that defeat Sir James South has not troubled the Royal Astronomical Society; and if there be any members who regret his absence, I can only repeat the remark of the noisy musician, who refused sixpence to move on (his fee being a shilling), "They don't know the value of peace and quietness."

I think it will be admitted that I had now given Sir James South sufficient provocation to set loose his tongue, if he had any disgraceful charge to make against me. But before this time he had "more dirt to eat," and partly at my hands. Probably some of the visitors will remember our encounters at the Board. I will here express my regret that, from the character of my opponent, I was compelled to be somewhat ruder of speech than would have been otherwise respectful to the Board. "You must flay a Russian," it is said, "to make him feel," and Sir James's powers of apprehension are of Russian coarseness. It must not be overlooked, either, that I was defending my friend the Astronomer Royal, Pond, who was worn out and in delicate health from the attacks of a person who had not the least notion of Pond's merits and services, and who could no more have given an account of the system of observation pursued at Greenwich, than he could compute a place of the moon. But though I did my honest endeavours to make Sir James's situation unpleasant to him I cannot claim the merit of driving him away. It was the calm and dignified rebuke of the Duke of Sussex which delivered us from our astronomical Trouble-all. Still not a word of the supposed conversations with Troughton.

And now came the quarrel between Sir James and his

* *Extract from the Minutes of General Meeting of the Royal Astronomical Society, Feb. 14, 1834.*

"A motion was made by Sir James South, and seconded by Mr. Hubert, 'That Lieutenant Stratford, as superintendent of the *Nautical Almanac*, is entitled to the sincere thanks of the Society; and as a pledge of their sincerity, this Meeting begs to recommend to the Council that he be presented with the Society's gold medal.' To which motion an amendment was moved by the Rev. Mr. Sheepshanks, and seconded by Captain Smyth: 'That the Society regrets the irregularity of the preceding motion, and hopes that it may not be repeated.' Which motion was carried unanimously."

"revered" friend Troughton, in which I took so active a part. I wrote the answers to Sir James South's letters,* which answers cannot be properly understood without taking my motives into account. I wished to keep him as quiet as might be, and to prevent any printing. His *cacoethes* of scribbling and printing was very distasteful to me. At the same time I comforted Troughton, encouraged Mr. Simms, and allayed the tempers of both, when they were tried beyond their endurance. Assisted by the advice of Mr. Airy and Mr. Donkin, Mr. Simms and I investigated the defects of the instrument (which had baffled the science of Campden Hill†), and discovered the remedies, which we then proposed to Sir James *on the condition, that if they failed, we were to make no claim for that part of the expense.*

Upon these conditions we proceeded to brace the polar axis; and though we were turned out before our work was completed, and though the object-glass was refused, we were completely successful (as far as we went) in curing the main fault, the tendency to *twist*.

Then followed another correspondence, but now Sir James prudently resigned his pen to his attorney,—a very fair man I dare say, but who could know nothing about the astronomical part of the subject, and only so much of the moral part as Sir James chose to tell him.

Our proposition was, that *we would put the instrument into working order at our own expense*; that it should then be used by gentlemen of the first reputation, agreed upon by both sides, who should decide upon its merits. This offer, though frequently repeated, was constantly refused. My belief is, that Sir James felt the decision would be adverse, and that he preferred to risk the chances of the law.

Troughton died while the suit was going on, and my conduct on the trial was more provoking than ever. I deposed positively

* Sir James printed this correspondence, but never published it; perhaps he made out that it would do him no service. From a loose sheet which I accidentally saw, he had mixed the letters with affidavits, &c. If I thought he would understand the appeal, I should call upon him either to publish or to satisfy me that the printed sheets were destroyed. I have been blamed for writing these letters, and writing underhand. I mentioned the fact to some common acquaintance—Mr. Baily, I recollect,—and I took for granted that the knowledge would pass to Sir James. There certainly was no concealment intended; and I did not write to inform Sir James, thinking it would look like a threat or insult. A good-natured, but not very discreet, friend, to whom portions of this correspondence had been read over the table at Campden Hill, assured me that he did not think them proper letters to have been addressed by tradesmen to a gentleman. "They are nothing of the kind," was my reply; "they are the letters of two honourable artists to a shabby, shuffling debtor." I fancy this natural view never occurred to people over Sir James's *Wine and Walnuts*.

† In justice to Mr. Simms, I must say that he had no opportunity for considering the instrument carefully until I had secured our undisturbed access. "Why don't you try so and so?" I once asked; and he replied, that he could try nothing and think of nothing at Campden Hill; for that the moment he arrived, Sir James and his family, and Mr. —, and the little dogs, all opened upon him at once, and fairly distracted him.

to the most essential quality of the polar axis, that it did not twist; I sorted and arranged the evidence and the cross-examination, Mr. Starkie, our advocate, taking up the matter most zealously, and giving me frequent friendly consultations. There is no vanity in saying that I spared Mr. Simms much worry, our attorneys almost all responsibility, and Mr. Starkie most of his trouble; and that if my evidence could have been tainted, my character compromised, or my personal attendance got rid of, the advantage to Sir James would have been immense.* Supposing the conversations reported in the *Mechanics' Magazine* to have been true, Sir James South could easily have embroiled Troughton and me by publishing them. Troughton was not a man to deny anything he had once said (Sir James, I think, dare not contradict me), and every one will see that I could scarcely have continued to be the confidential adviser of a man who had thus covered me with infamy. I *conclude*, therefore, that Sir James South's *inventions* are *not* anterior to Troughton's death; and I conceive every sane person, who has got a better head than a pin, will agree with me.

After Troughton's death all delicacy ceased—if, indeed, Sir James was ever troubled by such trifles,—and then I might have been assailed, but I was not.

It is true that Sir James South's recollection of conversations which had passed between himself and his “revered” friend became much more vivid after that friend's decease. He insisted so strongly on being examined as a witness, to depose to certain conversations, that the arbitrator consented, with the quiet remark, that a man's evidence in his own cause could not be allowed to reckon for much. Sir James tried to prove a sort of guarantee from a conversation between Troughton and himself, after the instrument was made, to the effect that it should be in every respect as good and manageable as his five-foot equatoreal; but I don't think any one believed him, and I am sure that Troughton never said anything which could bear so absurd a meaning, though he may have used some encouraging expressions to Sir James, at a time when his faith had been somewhat shaken.†

* In my cross-examination, which was in Troughton's lifetime, there was an excellent opportunity for introducing the attempt to suborn perjury; an opportunity which no lawyer or client would have let slip, *if it had been true*.

† When the large equatoreal was in a forward state, many of Sir James South's friends found fault with it, and frightened him; but not, I think, till the chief cost had been incurred. I think it not improbable that the words which Sir James considered a guarantee were spoken at such a time. I have a little doubt whether Captain Beaufort was present or no. There was a strong attempt to get him to depose to a guarantee, but he would not. Captain Beaufort's evidence in chief was so perfectly fair, that, when it was over, Mr. Starkie whispered to me, “He is our evidence,” and did not cross-examine. The truth is, that Sir James and his “revered” friend were rather at cross-purposes throughout. Troughton intended, as he told me before the instrument was begun, to make a *polar axis* for the telescope; the wood was to be of deal, and the declination circle a few inches in diameter—a mere finder. Sir James, who interfered, and directed, and meddled, and altered throughout, was always urging more expensive materials and work uselessly costly. “What would

As it was desirable to show the arbitrator what sort of a man we had for an opponent, Mr. Starkie cross-examined Sir James with a view to trot him out, and with perfect success. I suggested that he should be asked some questions about the reduction of equatoreal observations, and he certainly had some notion of index error; but when the question related to the corrections for parallax and refraction, we elicited the spark I was anxious for. Sir James exploded at once. "*Quousque tandem, Catilina, abutere patientiâ nostrâ*: a man who has got every quality of Catiline but his courage. I can perform those reductions as well as any man; I can perform them as well as the gentleman on your right" (I was on Mr. Starkie's right). "Shut us up in a room together, and I will prove it." I immediately said, "I accept the challenge, Sir James;" but I have vainly summoned him to make his boasting good. Sir James finds it easier to return a letter unopened, and to advertise the fact in the "*Times*," than to admit that he said rashly and under excitement what was not true.

In Sir James South's evidence, he asserted that, when the equatoreal was planned he proposed to Troughton to adopt Dollond's construction in the Cambridge transit, to save expense. I am *certain* that this is an untruth. From the time I first knew Sir James up to March 1831, he was most hostile to Dollond,* and it was one of his favourite employments to excite Troughton's bile against that very excellent artist by his "tale-bearing" and misrepresentation. Troughton, who was consulted about the instruments for the Cambridge Observatory, recommended that Dollond should make the transit, on the same model and scale as the Greenwich transit. For some reason or other, Dollond preferred another construction, and some of the members of this Board may, perhaps, recollect the fuss which Sir James made about the alteration. He would have had the instrument returned to the artist without trial, and spoke of the University as having disgraced itself by tolerating such an insult to Troughton.

Now as to Troughton's method of fastening a transit together, though it is very ingenious, it is not quite free from objection. I have no reason to think it need have been more expensive than Dollond's, if Sir James himself had not interfered and requested Mr. Donkin to turn the *inside* of the central sphere, an expense which

Gamby say to that?" was a common expression. He expressly ordered the central sphere of the transit to be turned inside and out, as I heard at the time from Mr. Donkin, who thought the expense useless. I was somewhat astonished to hear Sir James's expectations, knowing what Troughton proposed. When I remarked to him that the axis must bend, he seemed to think that Troughton would prevent it. As his memory is so good, he may, perhaps, recollect my warning: "Troughton can't alter the laws of Nature."

* Sir James, who was quite as anxious as Bob Acres to acquire a fighting reputation, used to say, that he should have been forced to call Dollond out, if he had been a gentleman; a curious exception for Sir James to take with respect to a man every way his superior. I never heard what was the cause of Sir James's wrath, but after his quarrel with Fleet Street, Sir James was most anxious to be received at St. Paul's Churchyard, and eat humble-pie accordingly.

neither he nor Troughton thought necessary;* and I am quite *positive* that Sir James never dreamed of any other construction as admissible, until *after* his quarrel with Troughton.†

I could point out, I think, other untruths, more or less demonstrable, but perfectly clear to me, in Sir James's examination and cross-examination. I will only repeat an anecdote which I heard from Mr. Simms soon after it happened, which will show what credit is to be attached to Sir James's recollection of conversations when less than thirty years have intervened.

Before the quarrel between Troughton and Sir James had become declared, but when the latter was very much dissatisfied with the great equatoreal, Mr. Simms went to Campden Hill to explain some old accounts which Sir James wished to settle. On passing through the observatory, Sir James remarked, pointing to his transit, "Ah, Mr. Simms, what a pity it is the old gentleman ever undertook an instrument after that!" Very soon after, as still further explanation was wanted, Mr. Simms went again to Campden Hill, where he found Sir James and his *fidus Achates* for the time, Mr. —. Sir James complained, as usual, of the great equatoreal, and then, turning to Mr. Simms, added, "And indeed, Mr. Simms, *as you so feelingly expressed it*, what a pity it is the old gentleman ever made an instrument after my transit!"

After we had obtained the verdict, and the money was paid, I gave myself very little trouble about Sir James South. The fate of the instrument may be learned from the following hand-bill, which was posted up rather extensively:—

* The sphere was cast in two pieces which were turned inside, and then tinned together. This operation, if I remember rightly, *cost* Mr. Donkin about 40*l.*, and was charged the same to Sir James. I think I understood that no profit was charged on any part of Mr. Donkin's work by Messrs. Troughton and Simms.

† In the *Phil. Trans.*, 1826, Part iii., p. 426, in a paper describing Sir James South's seven-feet transit instrument, the following passage occurs:

"By the above mode of joining the principal parts (*i.e.* Troughton's mode) the bars may be stretched, and the sphere even compressed to any extent short of that which would occasion a permanent alteration in the length of the former or in the figure of the latter; a thing which Mr. Troughton considers would, perhaps, not take place with a force equal to a ton of weight. How much such a connexion must be better than any that could be effected by binding together the *exterior* parts, to use the emphatic language of our artist,—'every one who is gifted with mechanical intellect will readily determine.' In a note to the word sphere it is said,—'That every part of the sphere should possess a power of resistance as uniform as possible: extreme precaution was employed in turning its interior surface so as to render it concentric with the exterior.'"

The description of the instrument is by Troughton, except a few interpolations by Sir James, such as the second sentence and the note in the above extract. I am by no means convinced that the central sphere of the seven-feet transit was turned in its interior surface. I remember that Mr. Donkin spoke of turning the interior surface of the central sphere of the great equatoreal as a novelty, which he did not think necessary, but which he executed at the special instance of Sir James South. There was some idea at that time that the telescope of the great equatoreal might be used as a transit, and it was accordingly made with great care and delicacy, as Sir James required. I suppose all this was forgotten by Sir James when he said in his evidence, that he had proposed to Troughton to follow Dollond's construction, *i.e.* that in which "the *exterior* parts were bound together."

OBSERVATORY, *Camden Hill, Kensington.*

To Shy-cock Toy Makers—Smoke Jack Makers—
Mock Coin Makers—Dealers in Old Metals—
Collectors of—and Dealers in Artificial Curiosities—
and to such Fellows of

THE ROYAL ASTRONOMICAL SOCIETY,

as at the Meeting of that most learned and equally upright Body,
on the 13th of May last, were enlightened by Mr. Airy's (the
Astronomer Royal's) profound *exposé* of the Mechanical Incapacity
of English Astronomical Instrument Makers of the present day.

TO BE SOLD,

By hand, on the Premises, by

MR. MACLELAND,

ON WEDNESDAY NEXT, DECEMBER 21st,

Between 11 and 12 in the Forenoon,

Several hundred-weight of Brass, Gun Metal, &c. &c.
being the Metal of the

GREAT EQUATORIAL INSTRUMENT,

Made for the Kensington Observatory,

BY MESSRS.

TROUGHTON AND SIMMS,

The Wooden Polar Axis of which, by the same Artists, and its Botchings cobbled
up by their Assistants,

MR. AIRY AND THE REV. R. SHEEPSHANKS,

were, in consequence of public advertisement on the 8th of July, 1839, purchased
by divers Venders of Old Clothes, and Licenced Dealers in Dead Cows and
Horses, &c. &c. with the exception of a fragment of Mahogany, specially
reserved, at the request of several distinguished Philosophers, which, on account
of the great anxiety expressed by Foreign Astronomers and Foreign Astronomical
Instrument Makers, to possess, when converted into Snuff Boxes, as a *souvenir
piquant* of the state of the Art of Astronomical Instrument Making in England
during the 19th Century, will, at the conclusion of the Sale, be disposed of,
at—per pound.

The want of truth, sense, and construction in this handbill re-
quires no notice; Sir James South had paid his bill, and like other
losers, had leave to pout. But I feel sure that if his "imaginary
conversations" had been invented *at that time*, they would have
been given to the world in some shape or other.

After this date, the latter end of 1839, I cannot recollect that Sir James had any very cogent motive to attack me, and his silence, therefore, does not prove much.* Now Mr. Babbage said, at our last Greenwich meeting in 1854, that he had advocated the charge against me as soon as he was acquainted with it: this, if literally interpreted, would imply that Sir James had not imparted the story to him much earlier than to the world in general through the *Mechanics' Magazine*. I should guess that Sir James did not fully *recollect* all the particulars of his story (*garnish* is, I believe, the professional word for this species of embellishment) before 1850 or 1851.

I am afraid that other persons, besides Mr. Babbage, will see something of a persecuting and malicious spirit in my conduct to Sir James South. I am not conscious of any such feeling. Throughout, I was defending some one else, or exposing an ignorant and mischievous charlatan, whose natural impulse is to worry other better men. My vehement indignation was first roused by his conduct towards Dr. Thomas Young, the most extraordinary man of the past generation. But there is scarcely an eminent astronomer, either English or foreign, whom this person, who cannot solve a plane triangle, has not insulted with his abuse at one time, and his praise at another. Young, Pond, Troughton, Pearson, Fallows, Baily, Stratford, Davies Gilbert, Kater, Woodhouse, among the dead; Herschel and Airy among the living. His first appearance was in the character of "a British lion," insisting that all the world should perform kootoo before the Greenwich Observatory and Troughton. He then took a Gallomania, and prated of Laplace, and Arago, and Gambey. Then came a German and Russian fever, and he became the worshipper of Bessel and Struve, whom heretofore he had held in small esteem. What right has a person of Sir James South's talents and acquirements to express any opinion about such men at all? I do not understand, and I trust shall never practise, the good manners which teach you to sit quietly by, when high and honourable men are assailed; and I think, and shall continue to think, that politeness of this kind arises from moral cowardice, and no better principle.

But though I took a very active part in plucking off the daw's stolen feathers, I will not claim more than my due. I have mentioned already how the Board of Visitors lost his company. It was a sarcastic remark from Davies Gilbert that drove Sir James South from the meeting of the Royal Society "five-and-twenty years ago," according to his pathetic speech the last anniversary, though he made no allusion to *that reminiscence*.

It was Mr. Gassiot, and not I, who styled Sir James a "calumniator," or some equivalent word, in a full meeting of the Royal

* I made a passing remark or two upon him, when I was defending the Liverpool Observatory, and setting Mr. Babbage right about the discovery of Neptune, and I *think* if he had had his story ready then, it would have been published.

Society, after the mask of the "contributing Fellow of the Royal Society" had been with some trouble detected and torn off. It was to shun the consequences of Captain Grover's wrath, not mine, that Sir James shut himself up in his house, and appealed to the Court of Queen's Bench for protection. I had nothing to do with Sir James's blackballing at the Royal Society Club,—an operation, if I remember rightly, twice performed. I had no hand in refusing him a Doctor's degree at Oxford, after the Chancellor had recommended him. *Suum cuique*; I may have been a "contributing member" to his humiliation, but I don't wish to rob my neighbours, or to publish their acts as my own.

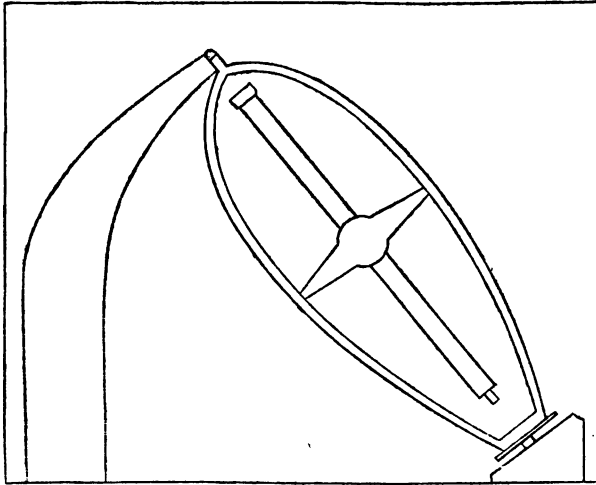
I will now leave the subject of Sir James South and his *credibility* to the judgment of the members of the Board, requesting them to excuse the length of these remarks, their want of connexion, and perhaps the plainness of the language. For the last failing, I might almost plead the words of Sir James himself: "I have not thought it worth while to polish the language in which the former (charges) were conveyed; they carry with them evidence of haste in point of composition, though they are derived from documents collected with considerable care; nor will I alter or soften down the style, to suit the taste of the fastidious; for if the charges themselves be founded on truth, words much stronger than I have used might be considered lenient."*

The slanderous letter of Sir James South in such a publication as the "*Mechanics' Magazine*," would scarcely have deserved any notice if it had not, like a bad bill with a solvent indorser, been backed by Mr. Babbage. To this gentleman I have been for some years a very bugbear,—one to whom he has ascribed all sorts of injuries, and calamities, and persecutions, and with such confidence, that I fear some unsuspecting people, ignorant of his malady, may believe him. I have no hope of removing his monomania, nor, so far as I am personally concerned, do I mind what Mr. Babbage may think of me. I know I should be very sorry to be tried for burning St. Paul's if he were on the jury at the Old Bailey. I fear he would find me guilty and starve upon it, though he had seen the church safe and sound the moment before he took his place in the box. There has never been any intimacy between Mr. Babbage and me, although we have many common friends. To say the truth, though I never had any doubt of his extraordinary talents

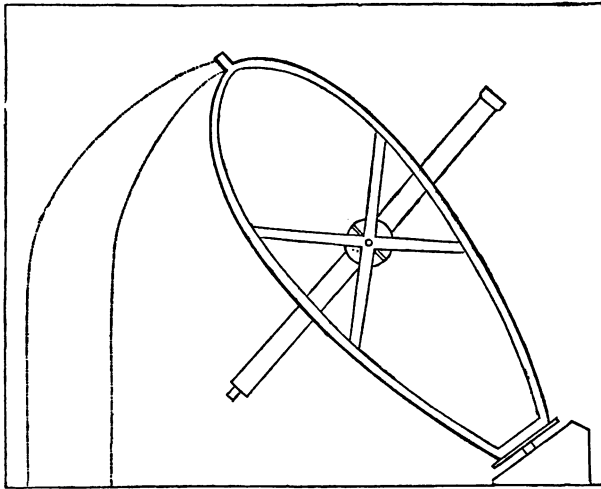
* See Preface to the second edition of "*Charges against the President and Council of the Royal Society*," by Sir James South, &c. London: B. Fellowes. 1830." In the advertisement, Sir James says: "It is well known to the scientific public that I stand pledged to present to them a work entitled, *On the Conduct of the Royal Society*," &c. It is at least equally well known that this pledge has never been redeemed. The advertisement proceeds to say that sanguine hopes were entertained that his promise might have been redeemed before the anniversary of 1830. "The unceasing attention, however, which the erection of my large equatoreal has demanded, will, I find, effectually preclude the completion of my wish." So at this time, Nov. 11, 1830, Sir James puts forward a claim to a sort of artistic partnership with the "revered friend," whom he wished "to do" so signally two years later.

in analysis and the science of symbols, I never could make him out to be anything but a very wrong-headed mortal on other subjects. As a member of a committee, he has always seemed to me

Great Equatoreal, as originally constructed.



In the Meridian.



At 6 hours from the Meridian.

a very crotchety person, who would not agree with any one else, nor propose anything with which any one else could agree. The origin of his dislike to me he has explained, though not quite

correctly, in his book on the Exhibition of 1851; and here I must enter into some particulars with respect to the great equatorial, for I cannot make matters clear otherwise.

I have given above two very rude diagrams of the instrument as it was originally constructed, and as we found it when we undertook its cure in the winter of 1832-3. The fault found with it was this:—when the instrument was turned a little round and then let go, a series of short, quick vibrations followed; there might be about a dozen vibrations, each lasting about 0·3 or 0·4 seconds.* These were very perceptible at the telescope or at the hour-circle.

I suspected, before I saw the instrument *in situ*, that the evil lay in the lower pivot, and that it arose from the interlacing, as it were, of the fibres of a small surface of contact under a heavy pressure. Mr. Donkin held a similar opinion, but thought the injurious pressure was on the small axes of two little friction-wheels, which formed the lower Y. I suspect now that he was in the right. The experiment which detected where the error lay was simple and decisive. A telescope was attached to the circumference of the hour-circle, and directed on a mark carried by an outrigger from the upper pivot. It was immediately evident that the polar axis moved *bodily* together, and that the cause of error was below the hour-circle. The cure I applied was a *broad* bearing of *soft* metal, when this vibration ceased.†

But now, when the instrument was moved round by laying hold of the telescope (it was quite steady when moved by the hour-circle), a vibration not unlike the former was discovered, and after a good deal of blundering (I am surprised at my own dulness) its cause was detected, viz., a twisting in the frame of the axis, which has evidently no provision against twist. This was a most unpleasant discovery; but I succeeded in diminishing the twist very sensibly by a rude bracing with pieces of quartering. The ultimate remedy was to carry deep pieces parallel to the hour-circle from one beam to the other beam of each cheek, to bind these deep connecting pieces together with deep diagonal bracing, and, lastly, to cover the outside with a thin sheathing of boards in the direction of the polar axis, and the inside with short, strong transverse boards, from beam to beam. The whole assemblage was strongly screwed and glued together by an excellent workman

* Sir John Herschel, I believe, suggested that the instrument might bind at the upper pivot, which would have agreed with the phenomena. Mr. Babbage's notion, which I heard from him at Cambridge, was this; that the whole frame was loose, and therefore, in every position, the centre of gravity fell below the line of suspension. Now, suppose the instrument moved a little, and the position of the parts fixed, the instrument would return like a pendulum. Granting Mr. Babbage this convenient alteration of fast and loose, we shall see that his hypothesis did not satisfy the appearances.

† If a good clock motion had been applied to the hour-circle, I believe no fault would ever have been found with the instrument: it would have required a very delicate hand, but that Sir James possessed. The first satisfactory clock for a large equatorial, that was known in this country, was one contrived by me for this very instrument. It was so steady and insensible to its work, that it only varied 6' in an hour, between carrying the equatorial and going quite free.

supplied by Mr. Donkin; and his expression, "it felt like a tree," was quite correct. The instrument now looked as if it were made up of two decked-boats, and in my opinion was handsomer than it was originally. Before this alteration, I had given the polar axis a definite twist, by applying two balancing weights, each of a hundredweight, at the upper and lower ends, and then applying them on different sides. I found that the double action of these weights twisted the axis through an angle of about 7'. *After the bracing*, the twist by the same weights, similarly applied, was insensible, but I am sure it could not have exceeded 7".

We were turned out in the autumn of 1833, and not readmitted until July 1834, when the arbitrator, being possessed of the cause, gave us a month, with the use of the object-glass (Sir James South and his friends being allowed to inflict their company upon us), with leave to affix our clock, and to procure evidence of what the machine really was as an astronomical instrument. Two-thirds, at least, of the month were consumed in making a screw (which, after all, was far from perfect), and in attaching and affixing the apparatus; but in the few days which remained, Mr. Donkin and Mr. Pond were shown the instrument, and were satisfied that it would do its work of measuring double stars. Mr. Airy, Mr. Simms, and I, made perfectly satisfactory observations *in distance* of three or four double stars,* although we had never, I believe, measured double stars before.

* Any equatorially moving telescope will measure *position*, though the observation requires dexterity and practice. We measured the angle of position chiefly in order to set the micrometer wires at right angles with the line joining the stars. The following list contains the results of *all* the observations, and the note-book in which they were entered was placed in Mr. Bethune's hands:—

§ <i>Boötis</i> .						
1834.	Position.	No. of Obs.	Distance.	No. of Obs.	Observer.	
July 18	53° 49' N. Prec ^s	3	3"·11	4	R. Sheepshanks.	
— 24	48 28	3	2·79	4	B. Donkin.	
— 48	0	1	2·82	3	R. Sheepshanks.	
30	51 29	10	2·98	10	R. Sheepshanks.	
			2·99	10	W. Simms.	

On July 25, Mr. Pond, Mr. Simms, and I, all took angles of position, but the results could not be ascertained, as the observations for zero had been neglected. The observations of July 30 in full sunshine.

ξ <i>Boötis</i> .						
July 11	57° 46' N. Prec ^s	1	—	—	The screw very imperfect.	
18	58 13	4	7·16	3	R. Sheepshanks.	
— 59	19	5	7·30	4	W. Simms.	
30	60 16	15	7·30	10	R. Sheepshanks.	
— 59	14	5	7·49	10	W. Simms.	

α <i>Herculis</i> .						
July 24	27 39 S. Foll ^s	1	4·95	2	R. Sheepshanks.	
— 28	31	11	4·93	8	Prof. Airy.	
30	28 35	5	4·95	10	R. Sheepshanks.	
— 29	37	5	4·92	10	W. Simms.	

61 <i>Ophiuchi</i> .						
July 30	3 37 S. Prec ^s	4	20·47	4	R. Sheepshanks.	

Several of these observations were taken when the star was nearly two hours from the meridian. The power used was about 240, as high as the telescope would well bear, except on July 30, when Mr. Simms and I used 500 for α *Herculis*.

In their examination, Mr. Pond and Mr. Donkin said they had no doubt of the capability of the instrument to do its proper work, viz., make micrometrical measures. Mr. Airy said he could measure a double star with it as easily as he could measure two dots on a sheet of paper. I was quite willing to let its quality rest on the distance-measures of MM. Airy, Simms, and myself. I may add, that Dr. Robinson, the only astronomical witness produced by Sir James South, admitted that the apparatus was sufficient and convenient for taking sets of measures of double stars; but he complained that it could not be easily set on a star, and that the place of a star could not be correctly ascertained by it. I can only reply, that he exhibited several nebulae to the arbitrator which were found by the setting of Sir James's gardener, Lyall. Moreover, that Dr. Robinson's observations (made at my request), when the obvious errors of reading were corrected, and the instrumental errors deduced and applied, did not, I think, exhibit a probable error of 30" or an extreme error of 90", though some stars were observed at six hours on each side the meridian.* Now this is far more than ever Troughton expected, for he considered the circles as mere finders, and the declination circle was increased from nine inches to two feet, chiefly to get proper leverage for setting. The edges of the radii were rounded on this special account. If Sir James South would have allowed us to finish the instrument, which we offered to do at our own expense, and would then have submitted the matter to independent astronomers, who were no partisans, this long and expensive litigation would, I conceive, never have arisen. I don't think that any honest man could have refused our proposal, nor can I think any compromise would have been reasonable until our proposal had been tried.

In our invitations to gentlemen to inspect the equatoreal in July 1834—though even then it was not so good as we think we could have made it—we included Mr. Baily and Captain Smyth, who might at that time be considered as neutral; and Captain Beaufort and Mr. Thomas Jones, whom we knew to be hostile. I cannot have known that Mr. Babbage was to be a witness, or I should have invited him, notwithstanding his small acquaintance with, or aptitude for, astronomical observation.† MM. Baily and Smyth declined, I think, unless they were invited by both sides. Captain Beaufort declined, as being Sir James's friend; and Mr. Jones gave, I believe, no answer. Several other persons were

* I may perhaps have failed in my computations (the principles of which will be found under EQUATOREAL, *Penny Cyclopaedia*), but this is the conclusion I arrived at, after carefully working out the whole of the observations twice over. I could not, however, *prove* this on the trial, as my examination was over almost a year before Dr. Robinson's observations were made. I offered to publish the observations and my results for public examination; but the defendant's attorney and counsel *objected most strenuously*.

† I am sure that Mr. Babbage is strictly true as to matters of fact occurring within the perception of his senses (except perhaps in what regards me), and I think that I could have taught him to measure the distance of a double star with our apparatus, though perhaps not very well. We were at that time on good terms, as good as we had ever been.

asked, but except those whom I have already mentioned, and Mr. T. Bramah, no one came.* It was, unfortunately, cloudy on Mr. Bramah's night, and he saw nothing; he *heard*, however, a good deal of reproachful language from Sir James South, who made no secret of his intention to consider the seeing the equatoreal under our care as a personal injury and insult.†

Mr. Babbage gave his evidence against the instrument, and as it seemed to me with considerable bitterness, some time before the long vacation of 1835. I shall consider the substance of his evidence in chief, and in cross-examination, a little further on; but the "unextinguishable hate" which he has borne me for almost twenty years dates from a conversation which took place at that time. As there were a few minutes to spare after the examination in chief was finished, Mr. Starkie, who was not prepared to cross-examine, asked Mr. Babbage some questions as to his intimacy with Sir James South, and how often he had dined with him, &c., rather too much in the Skimpin style to please me. This irritated Mr. Babbage, who had reckoned, I suppose, on more courtesy from a brother professor; and he came up angrily to Mr. Starkie and me, after the meeting was over, to complain. Now at the moment, I too was full of wrath, not at the effect of Mr. Babbage's evidence (for I saw its weakness, and how much it might be turned in our favour), but that a man of Mr. Babbage's reputation and liberal professions, the Lucasian Professor, should so expose himself. He disliked Troughton (so I believe) for no better cause than that Sir James, who quoted the old man "with additions" on all occasions, has frequently put him down, and very offensively, by Troughton's authority.‡ I believe, too, that Mr. Babbage was glad to seize upon an opportunity to go against Mr. Airy, whom I suspect he never forgave for

* Mr. Simms has supplied me with a note of the gentlemen invited by Troughton and himself to inspect the instrument:

Capt. Beaufort,
Professor Barlow,
Dr. Gregory,
Mr. F. Baily,
Mr. T. Bramah,
Mr. Cubitt,
Mr. T. Jones,
Mr. E. Riddle,

Mr. Dollond,
Mr. J. Penn,
Mr. B. Donkin,
Mr. Pond, A.R.
Capt. W. H. Smyth,
Mr. Henderson,
And also the Defendant
and his Friends.

† Mr. Bramah was greatly surprised at his reception by Sir James, and remarked to me, that though he had often been called upon to inspect the subject-matter of a dispute, this was the first time he had been so treated. I own I should have been more surprised if Sir James had "behaved himself." Yet I have no doubt Sir James quite agrees with Mr. Babbage, that the habit of stopping evidence by threats is peculiar to *me*.

‡ For years it was Sir James's habit to go backwards and forwards between Troughton and his other acquaintance, repeating and improving what each might say disparaging of the other. In this way he stirred up a very fair amount of bad blood, and especially, I believe, in Mr. Babbage, of whom at that time he thought very little. A horse-laugh, and "Troughton says," formed his triumphant reply to every remark; and I am sorry to say that some most excellent and sensible men did not see through the trick. I was one day discussing a point of astronomical construction with one of the most able and candid men this

being preferred as Lucasian Professor, and whose discharge of that office formed such a contrast to his own. I may be fanciful perhaps, and uncharitable, but I was of opinion then, and am so still, that Mr. Babbage *calculated* that he was playing a safe game. The arbitrator was a friend, to whom he owed, if I have not been misinformed, some germs of his speculations on Functions; Mr. Starkie was a brother professor, and Mr. Bethune, a friend and admirer. Be this as it may, there is no doubt I was a good deal excited when I heard such evidence from Mr. Babbage, who not long before had offered himself for Finsbury (and I had voted for him) on the especial ground that he would promote the interests of the industrious and intelligent classes. I do not think I should have sought the occasion of speaking my mind; but when he had thus offered it, undoubtedly I availed myself of it with the utmost freedom, it may be with some rudeness. I told him that he cried out before he was hurt, and that his cross-examination would give him far more reasons for complaint. That he had disgraced himself that evening doubly; by his mechanical ignorance, and by supporting a person whom he knew to be a charlatan, and who was also dishonest enough to refuse payment, while he denied the artist the liberty to finish and prove and exhibit his work. I said that I did not think any gentleman who knew the circumstances had any right to assist Sir James South, until he agreed to act honestly; adding, that if my clients had shown the least disposition to do less than justice to Sir James, I would not have aided them a moment longer. Mr. Babbage admitted that he thought Sir James wrong in that respect, and that we ought to have had leave to finish our work; but he maintained that he himself was justified in the course he had taken. After a little more snarling, I told him that I would expose his ignorance, and show him up; and to his reply, that he did not care, I told him I would make him care.

All this will seem very coarse and rude, the previous excitement on both sides not being considered. As far as I remember, we walked quietly up and down Lincoln's Inn Fields during the talk, and I allow, that, though I think myself undoubtedly right in the view I took, the vehemence of language was all on my side.

As was, perhaps, natural, this conversation made a very different impression on the two parties. I had no idea that Mr. Babbage could be much affected by it, until I heard that he was so

country can boast of, and to strengthen my own view had just said, "Troughton's opinion is," when I was stopped by a sudden interrogatory, "Before you proceed, let me ask, do you swear by Troughton?" "No," was my reply; "but he is a very able, sound-headed man, and has devoted a long life to this peculiar subject, and I think his opinion is always of value." "Most certainly" was —'s answer, and we ended most amicably, and in accordance with Troughton. Sir James misused Troughton's name as much as Falstaff did the king's press; and one of the rods I most frequently applied to him was to state Troughton's *real* opinion in contradiction to his erroneous version.

from our common Cambridge friends, to whom he made very bitter and very womanish complaints. But I was most unfeignedly surprised, when at our next meeting for his cross-examination, Mr. Babbage appealed to the arbitrator for protection, with a very absurd mixture of pride and fear. He said that I had threatened him and other witnesses in this cause (not naming them, however), and he had seen such consequences of my malignity in the case of Sir James South, that he wished to guard himself from the like. Not that he cared, however. The arbitrator asked me what I had to say in reply. I answered, that what Mr. Babbage said was true,—I had threatened to show him up, and what was there to prevent me doing so if I chose? Mr. Babbage was not the man to complain of being exposed. But I would insure him against the fear that I might attack him underhand. I said that I had never spoken or written anything that I was not ready to avow, and that it should never be said of me, that I walked arm-in-arm with a man as his friend, having a printed libel against him in my pocket.* The arbitrator simply remarked, that it was agreeable to find, what indeed might be expected, that two gentlemen should agree as to what had occurred, and then directed Mr. Starkie to proceed.

I have already mentioned Mr. Babbage's unlucky hypothesis as to the cause of the small vibrations in R.A. which were found in the Campden Hill Equatoreal. I don't think his fast and loose principle will find much favour; but there was an obvious mechanical objection to his solution, which the occupier of Newton's chair should not have overlooked. Huyghens discovered this property in every body supported on an axis, that the distance of the centre of gravity from the point of suspension multiplied by its distance from the centre of oscillation, is a constant quantity, and equal to the square of the distance from the centre of gravity to the centre of gyration.† Now if we suppose the centre of gravity of the polar axis to fall 0·2 inches below the axis of suspension, and the distance of the centre of gyration from the centre of gravity to be 12 inches, the other term in the proportion will be 12^2 divided by 0·2, or 720 inches, i.e., the oscillations in this case would be synchronous with a pendulum of about 60 feet. Now the actual oscillations corresponded to a pendulum of about 6 inches. An exposition of this mistake formed the commencement of Mr. Babbage's cross-examination; but whether Mr. Babbage was discomposed by the failure of his charge of intimidation or not, it is certain that he would neither recollect nor deny his hypothesis. What was still more extraordinary, he did not evince any acquaintance with Huyghens'

* I have been told, on what seems to me very respectable authority, that Mr. Babbage walked amicably with Capt. Sabine while the "Decline of Science" was in the press.

† On this property depends Kater's measurement of the length of the simple pendulum.

principle, though it was quite familiar to the arbitrator, to Mr. Starkie, to Mr. Bethune, and to myself.*

Mr. Babbage was well acquainted with the measures I had taken to prevent the twisting of the polar axis, and he might have learned from my evidence that they were perfectly successful. I should say that no ordinary mechanic could have doubted of the general effect of the bracing, though he might not have been able to see the exact extent. Mr. Babbage could not see any principle, however,† nor that the connecting pieces and braces were contrived to keep all the parts in their places. His definition of a tie was that it was "a long, thin, piece of iron," and he could not see that our deep cross-pieces, which connected the two beams of each cheek, acted as struts to keep the beams apart and ties to prevent their further separation.

Mr. Babbage pointed out a real failing, which Mr. Simms and I were well aware of, and which could have been remedied in a great degree, if remedies had been required or permitted. This was the wriggling at the base, especially when the telescope was six hours from the meridian. In this case the upper cheek tends to straighten, and to push out the upper side of the base,‡ while the lower cheek bends outwards, and so shortens, and pulls in the lower side of the base. But in an astronomical instrument of this nature, which was designed by the maker for micrometrical measurement and *nothing else*, the derangement arising from this cause could scarcely be injurious. It would have been allowed for if necessary, I think, by applying a slight addition to the latitude correction, varying according to the hour-angle. Mr. Babbage, however, furnished no means for estimating the amount of this wriggling, and Dr. Robinson's observations show that it must have been small. Another experiment, I think, was produced by Mr. Babbage to prove that the cheeks approached each other at their lower edges when in the meridian. Laths were inserted between the cheeks at their lower edges, which fell out when the instrument was turned round. That

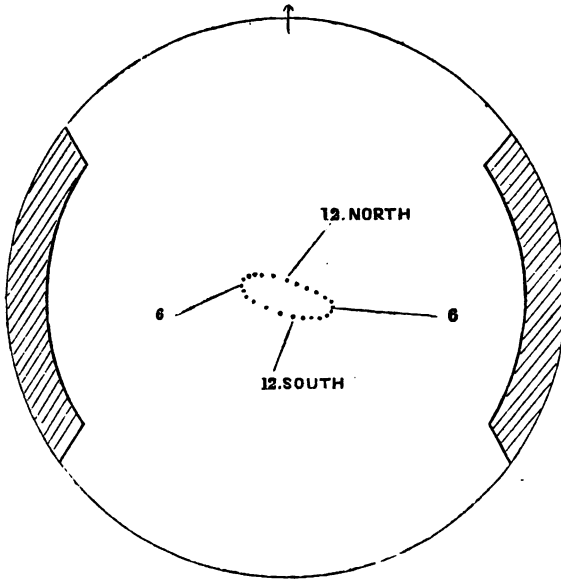
* I have no doubt that with time, and in a quiet place, Mr. Babbage would have recognised his hypothesis and its folly; but he is an unready man and easily flurried, and I am not quite sure he comprehended what was going on until it was over. That was no concern of ours; when we felt that the arbitrator was convinced, we did not stay to convince the witnesses that they had "blackened their own faces," which was generally the case.

† None of the witnesses except Dr. Robinson attempted to speak of this machine as an astronomical instrument. None were aware that its original defect was a liability to twist, and nearly all declared that the bracing had no principle *that they could see*, which I have no doubt was true. The main objection taken was to the great bending of the axis, which, it will be seen, did not exist, and, if it had existed, would have been of small astronomical importance, as it would scarcely have deranged the *direction* of the telescope, *which is the only essential matter*.

‡ The base should have been much thicker and continued downwards to end in a blunt cone, like the Schuckburgh Equatorial and the 5-foot Equatorial at Campden Hill. Strong metal plates, connecting the cheeks and the base more securely, would have lessened the defect and could have been applied easily.

this must be the case is obvious enough ; but, owing to the symmetry of the instrument, this yielding had no *astronomical* effect. The position of the declination axis in space, if altered at all, was shifted in a direction nearly parallel, and the astronomical effect was not sensible. An attempt was made by an earlier witness to impeach the strength of the machine and to insinuate that it might work itself to pieces ; but this notion arose from his entire ignorance as to the strength of materials and the laws of elasticity, and everything else. With proper care, the great equatorial would have outlived the present generation and two or three more to come.

After exposing Mr. Babbage's fast-and-loose hypothesis, and its supposed consequences, he was cross-examined as to an experiment of his own. (*See Figure annexed, in which the shaded portion signifies a section through the two cheeks composing the polar axis.*)



The telescope was pointed to the pole, and the declination axis firmly clamped ; the eye end was prolonged downwards, and ended in a needle or pencil. The instrument was turned round in R.A., and at every hour a mark was made in a piece of paper fixed at the middle of the base. This drawing was produced by Mr. Babbage at his examination in chief, as a proof of the great bending of the polar axis, and it was upon this experiment,*

* Mr. Babbage was unlucky ; a previous witness had brought this experiment forward, which, by Mr. Airy's help, I had completely analysed, and saw that it contradicted the opinions of the experimenter.

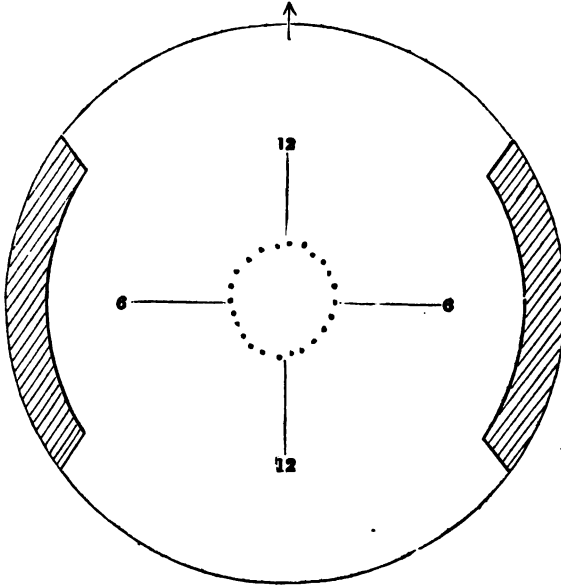
which he clearly did not understand, that I had chiefly relied for "showing him up."

Anybody who considers the experiment carefully will see that the dotted curve is made up of at least two elements, the flexure of the lower half of the telescope, and the bending of the axis. Mr. Babbage had already given us the value of the flexure of the telescope. So having first entrapped him into an opinion that the polar axis bent more than if it had been made of straight timber,* though he admitted he did not know how much straight timber would bend, it was explained to him that the minor axis of the curve was made up of twice the flexure of the lower end of the telescope and twice the bending of the axis in the meridian. This he admitted; and, indeed, it is quite obvious. It was then explained to him that, according to his own data, the single flexure of the lower end of the telescope with its continuation was at least .08 inches. This, too, was admitted. He was then requested to subtract twice this amount from the minor axis, to halve the remainder, and tell us how much the polar axis bent, *from his own experiment and on his own data*; and I handed him a pair of compasses and a scale to perform the operation. "According to this," was Mr. Babbage's stammering answer, "the axis does not bend at all in the meridian; but I can't be expected to know so much about the instrument as those who have spent so much time over it." As he left the room in considerable discomposure, I own I did not envy his feelings. On looking at the major axis of the curve, it will be seen that other elements enter; the end-shake of the declination axis, and the angular effect of the base-wriggling on the declination axis. The end-shake is, astronomically speaking, of no importance. The declination axis was designed, if I remember rightly, to rest on one cheek and to hang from the other when the instrument was out of the meridian; and I believe the adjustment might have been so made as to diminish the end-shake, and, consequently, the major axis of the curve considerably. Certain it is, one of the suspending caps acted, and the other did not; but we were allowed no opportunity for correction.†

* This opinion was shared by almost all the Campden Hill mechanics, while they admitted candidly they did not know, and could not calculate, how much the polar axis did bend, or how much, if made of straight timber, it might be expected to bend. There must be a little hypothesis as to straight timber; but I think I found from Tredgold's formula that a straight beam of the length and scantling of the polar axis beams, placed at the same angle, and loaded with one-fourth the weight of the telescope, would bend more than 0.6 inches.

† The effect of end-shake is very evident in Mr. Babbage's diagram, the points are most widely separated at two or three hours from the meridian, where the variation of flexure in the axis must have been very small. The astronomical effect of end-shake, I need not say, is nothing. In explaining the mode of fixing the ends of the declination axis, I made a mistake, confounding the impression I had received from Mr. Simms' description, with the matter of fact. Some attempt was made to discredit me on this account, though I admitted the mistake directly it was pointed out, and explained how the confusion had arisen, and although the fact had the same bearing either way. The unequal wearing of

An experiment was devised and tried by Sir William Cubitt, which, when carefully analysed, throws great light on the constitution of the polar axis. He fixed the object end carefully at the upper pivot, and obtained the following curve from the needle, or pencil attached to the eye end, while the axis was turned round :—



In analysing this curve, it must be remembered that the telescope is *balanced* while the upper end is *fixed*; and therefore that, in the meridian, the drop of the needle point due to the flexure of the telescope, is nearly double of what it was when clamped, being the *sum* of the curvatures of the object and eye ends of the telescope. The diameter of the curve in the direction of the meridian is, therefore, twice the flexure of the upper end of the telescope + twice the flexure of the lower end and its continuation + twice the bending of the axis. This was taught me by the Astronomer Royal; and after a little explanation was clearly seen by the arbitrator. Mr. Starkie then assumed, as a self-evident truth, what, indeed, I had impressed upon him, that the same

the caps was in favour of the instrument, for it showed that it was affected by a corrigible error, which a proper adjustment would have destroyed. This unequal wearing had been given in evidence on the defendant's side, and not disputed. When we were admitted by the arbitrator, we looked at the caps, and, finding one a good deal ploughed and worn, Mr. Simms asked me if it would not be well to take it home and planish it, and I advised him to do so. Out of this simple fact, Sir James and "his tail" raised a host of monstrous and malicious surmises, which had no sense or probability to support them.

exposition would serve all round ; and that, therefore, however absurd it might appear, the polar axis must be almost equally stiff in every position. But the arbitrator immediately pointed out the essential difference between the statical condition of the telescope in the meridian and at six hours from it. At six hours, the bending of the upper half of the telescope has no effect on the lower half, which would remain in the same place if the upper half were cut off. This just distinction, in a principle which had only been recognised a few minutes before, strikes me even now with surprise and admiration. It is clear that the diameter of the curve at right angles to the meridian is made up of twice the flexure of the lower end of the telescope, + twice the bending of the axis at six hours, + twice the end-shake, + whatever angular effect may be due at the declination axis to the wriggling.

Though the bending in the meridian by Sir William Cubitt's experiment is rather larger than Mr. Babbage made it, the astronomical effect in a micrometrical instrument is altogether insensible.

In estimating the components of Sir William Cubitt's curve, we assumed the bending of the two ends of the telescope, using Dr. Robinson's data, to be about 0.2 inch, so that 0.4 of the meridian diameter of the curve is due to the bending of the telescope. The whole effect of the telescope bending in the direction at right angles to the meridian we found to be about 0.25. It is not possible to assign the amount of end-shake, but it is clearly very considerable. There was, too, I believe, more play in the sockets of the declination axis than ought to have been. I conceive that in the meridian the axis did not bend so much as 0.1 inch, and at six hours from the meridian scarcely more than 0.15 inch.

It is for Sir James South and his coadjutor, Mr. Babbage, to explain why an instrument so convenient and capable of such good work was wantonly destroyed. The owner of any inanimate thing has a right to do what he will with his own ; and as Sir James paid for the equatoreal, *to the last farthing*, he had the same right to break it to pieces as a child has to smash his toys. But if Sir James and his friend, having got this material witness out of the way, assert that the great equatoreal was not well adapted for its designed object, they will meet small credence from the astronomers and mechanics who have attended to the evidence which I have just adduced. Perhaps the fact that Sir James was ordered to pay, not merely for the instrument as originally designed, but for my alterations, *which were not to be paid for unless they succeeded*, may satisfy persons who are neither astronomers nor mechanics, that Mr. Babbage's assertion, "that no after contrivances or expense could correct the errors of an instrument itself radically defective in principle," is a simple falsehood. So much for Sir James South and his great equatoreal.

I will now examine in detail the calumnies which Mr. Babbage has vented against me in his book entitled *The Exposition of 1851*.

I have no intention of criticising the whole of this farrago, in which what is true is not new, and what is new is mostly not true, but merely the twelfth chapter, headed *Intrigues of Science*, which contains several pages consecrated to my especial annihilation. Notwithstanding its catchpenny and delusive title, the work excited little attention; and I first heard of the honour which had been done me, from the Astronomer Royal, some weeks, if not months, after the appearance of the volume. Not holding with Mr. Babbage, that whatever is asserted and not disproved must, therefore, be true (nor caring much for those people who do hold such an opinion), I put off my reply to a convenient opportunity, when I should have more leisure: but I was so confident that everybody acquainted with the Astronomer Royal or myself, or familiar with men of science, or astronomy or astronomers, would see the absurdity and falsehood of such accusations, that, without further provocation, I might have neglected it altogether.

The title of the work called *The Decline of Science*, which Mr. Babbage published in 1830, and which he alludes to at starting, is a misnomer. Science was not declining in England in 1830, but rapidly rising. I have never heard this disputed by any one worth citing. The title was, I believed, cribbed from Davy, and the book did not answer to it. It should have been called, "Mr. Babbage's Revenges against all and sundry whom he considered in any way concerned in electing Captain Sabine as Secretary of the Royal Society, and in rejecting him." Mr. Babbage wished to be Secretary, and he conceived that the President (Davy, I believe) was favourable to his appointment. Whether Davy changed his mind, or whether Davies Gilbert, who succeeded him, did not hold himself bound to carry out the intention of his predecessor, or whether Mr. Babbage deceived himself, I do not know; but I do *know*, for I heard it from Mr. Babbage, that he quarrelled with the Royal Society *because* it did not get rid of the President who had done him such an injustice as to reject him as secretary. He had a notion, too, which struck me at the time as a very crazy one, that there was some underhand dealing, and that he had been invited to be a candidate to give *éclat* to Captain Sabine.*

With this key, I think "The Decline of Science" will be much more intelligible to the general reader (if any reader there

* I heard from Sir James South that Davy had invited him to become Secretary, but that he retired in favour of Mr. Babbage. I think the Royal Society had a lucky escape in both. But Juno herself did not take her affront more to heart than Mr. Babbage, nor for a longer period. His anger extends to all who have held the same office as Captain Sabine, and to his very profession. If it ever came to Mr. Babbage's knowledge that Davies Gilbert made some overtures to me to fill that office, his indignation must have been excessive. I am not ambitious of occupying situations for which better men can be found, and, therefore, very respectfully declined coming forwards, notwithstanding the temptation.

be of that work) than it has been hitherto. The astronomical criticism is understood to have been borrowed.*

Mr. Babbage next *assumes* that, because this silly and ill-natured production was not specially answered, the facts are indisputable: and he quotes a remark from Francis Baily, that Professor Moll's pamphlet was "an admission of the truth of his statement." With, perhaps, one exception, I don't know that anybody was interested to reply. Most gentlemen think it better taste to leave libels unanswered; and, though I think the policy doubtful, considering the ignorance of most readers, and the ill-nature of many, the rule is followed by the best men. Those who are conscious that they may despise paltry accusations, are often too proud to descend to such ignoble strife. I should interpret the silence with which "The Decline" was received, to carelessness or contempt rather than to conviction of its truth or importance. Its effect was nothing among men of science, though it probably led the way to Sir James South's "*Thirty-nine Charges*," which, in like manner, have never been contradicted, that I know of, though they have been ridiculed and laughed at.

I do not believe that a sensible man like Francis Baily could have spoken of Professor Moll's pamphlet in the sense in which Mr. Babbage quotes him. I conceive that Mr. Baily alluded to the domestic management of the Royal Society, or to some of those local topics which Professor Moll never touched upon, and which most interested Mr. Baily at that time.

The Professor's object was to set this country, which he liked, in its proper light; to prove that there was no decline in its science, but the contrary; and to show that, if the rewards of men of science in other countries were in some instances larger in amount, or more profusely distributed, than with us, yet that they were not unfrequently purchased by political servility. Beyond these points, *which he proved*, Moll's answer did not profess to go. I have never heard any one doubt, except Mr. Babbage, that on these points Moll had a decided advantage; and, indeed, according to Mr. Babbage's principle, that what "has never been disputed" is true, Professor Moll's pamphlet must be believed, for it has not been controverted.

In the next pages Mr. Babbage expresses his opinion that "They order this matter better in France," and that the Institute is a far more efficient scientific organ than the Royal Society. It is not my concern to dispute this position, though it might demand some limitations and qualifications; but I may remark, that if the people of England wish to have an Institute, and *are inclined to pay for one*, there is nothing to prevent them. The Royal Society is a voluntary and self-supporting body, which costs the public next to nothing, and which yet has "done the state

* So I have heard, and I believe it, for there are inconsistencies which look like two hands. It is, however, so superficial, and in general so inconclusive, that it *may be* the sole production of its presumed author.

some service" in the opinion of more competent and less prejudiced judges than MM. Babbage and South.

In England science is not a *profession*, as it is on the Continent, and with our expensive social habits on the one hand, and the temptations held out to men of talent, on the other, by the law and the church, it would be no light undertaking to make it a *competing* profession.*

It does so happen that the Secretaryship of the Academy of Sciences at Paris is the highest scientific honour in that country, and that with us we think more of the office of President. If the Royal Society cannot at all times command the highest and most matured talents for its secretaries, there is no blame that I can see, for it certainly would take them, if they were offered. There are only a few men of science who can afford the time required by the duties of secretary; and if the office be efficiently filled, it is hard to say where there is any grievance. Mr. Babbage does not allege any incompetence in the officers of the Royal Society, but simply that they are not such distinguished men as the secretaries of the Academy.†

* The simple and unexpensive habits of the Continent, the numerous universities and professorships which exist there, and the small encouragement given to other courses of study, must not be overlooked by any one who wishes to understand the matter so crudely propounded by Mr. Babbage. The English universities have few professorships, and those, generally speaking, very poorly paid. If Mr. Justice Maule had preferred science to law, he could not have reckoned on as many hundreds a-year as he has now earned thousands. It is the undoubted right of the people of this empire to say, through their representatives, what they are desirous to have and are willing to pay for; but the tone of complaint and grievance adopted by some *soi-disant* friends of science is truly disgusting. It is a subject for calm inquiry and careful deliberation, and not for childish querulousness.

† The following is a list of the Secretaries of the Royal Society for the last fifty years:—

Elected.	Resigned.
1804 William Hyde Wollaston	1816
1807 Humphry Davy	1812
1812 Taylor Combe.....	1824
1816 William Thomas Brande	1826
1824 John Frederic William Herschel	1827
1826 John George Children	1827
1827 Peter Mark Roget.....	1848
1827 Capt. Edward Sabine, R.A.	1830
1830 John George Children	1837
1837 Samuel Hunter Christie	1854
1848 Thomas Bell	1854
1854 William Sharpey	—

A list which most persons will look at with respect, though it may not satisfy the critical eyes of Mr. Babbage. The office is laborious and responsible. Is it quite certain that men of the very highest class would always be most useful to science as secretaries of the Royal Society? I fancy that Faraday, Airy, Owen, &c. are perhaps as advantageously employed at present as they would be in carrying on the business of a Society, much of which is routine. The depreciating remarks of Mr. Babbage remind one of the Fox and the Grapes. Mr. Babbage was Secretary of the Astronomical for some time, but our books show that his colleague, Francis Baily, did the work. If Professor Stokes takes Mr. Christie's place, the country can supply no better man.

There is a point of view in which the *mixed* nature of the Royal Society may be considered, which seems to have escaped most of our grumblers; it is that the union of so much rank, wealth, talent, and even of numbers, gives it great and appropriate weight in such a country as England. The Royal Society provides a competent body to inquire into every discovery and gives immediate and extensive publication to whatever is found to be of value. It is no slight advantage to be backed by the goodwill of eight hundred gentlemen, who comprehend almost all the scientific talent of the empire; and if national aid should be required, the influence of the Royal Society, with a good cause, is almost paramount.*

In reference to Mr. Babbage's admiration for the social position of men of science in France, I will quote one passage from Moll's pamphlet. After pointing out the fact, that the incomes of leading men of science under Napoleon were purchased by political subserviency, he adds, "If we wish to know what the *emolument of science* is in France, let us recollect the instance of Legendre, certainly a real deserving and meritorious man of science. By an arbitrary act of ministers, he was deprived of a scanty pension,—his all,—and for what? Because he did not choose to vote for a ministerial candidate for member of the Institute."

At page 152, an allusion is made to a discussion which took place a few years ago at the Royal Society on some irregularity in an award of one of the royal medals. The irregularity was, if I remember rightly, very candidly admitted, and as there was no real injury done, and no harm meant, the Society in general was, I believe, satisfied. That all parties should agree was scarcely to be expected, but I do not allow that "the Council escaped censure, in consequence of some little want of management in those who proposed it." The fair, and, I believe, true conclusion is that which I have just stated. Mr. Babbage then goes on to say:—

"During this discussion, one of the Fellows of the Royal Society got up and remarked, that although this case was very bad, it became trifling when compared with the circumstances

* If the Royal Society does not employ this power well, the fault is in the *persons* of the body, and not in its nature. That it has been sometimes unwise in its action is likely enough, and sometimes unlucky. As an instance of its want of wisdom, I may mention the recall of Mr. Rümker from Paramatta, and sending Mr. Dunlop in his place, which I believe was its act; as a specimen of bad luck, I may instance its recommendation to the Government to carry Mr. Babbage's Calculating Machine into execution without some guarantee. The real defect in the Royal Society appears to me to be in the composition of its Council, half of whom must, by statute, be changed every year. A body so frequently decomposed cannot be expected to work with unity or steadiness of purpose; and if there be any justice in Mr. Babbage's surmise, that the permanent officers have too much power, it is a necessary consequence of their being the only persons who have the habit and traditions of office. It is, I conceive, impossible to have efficient Councils where the persons are so frequently changed. As to Jobbery, that is a mental delusion in Mr. Babbage, and vulgar slang in Sir James South, so far as my knowledge goes; yet I am no blind admirer of the Royal Society.

attending the very first award of the royal medals; for on that occasion the Council had wilfully violated the laws they had themselves established for their distribution, and that on his formally demonstrating the facts by reference to their own minutes, they with singular consistency refused to alter their unfair and unjust decision." The speaker was Mr. Babbage himself; but to the best of my recollection, he was stopped by a call to order as soon as it appeared that he was going to waste the time of the meeting by reference to past matters which were not before it; if he had been heard, he would undoubtedly have spoken to the purpose here cited, and so shown very decidedly *his* want of management. The circumstances attending this first grant of the royal medals has probably some connexion with Mr. Babbage's animosity against the Royal Society, and I should date, conjecturally, the *commencement* of "Achilles' wrath" from the year 1826.

Sir Robert Peel, in a letter dated Dec. 3, 1825, informed the President (Davy) that the King would place annually two medals of fifty guineas each at the disposal of the Society.* The Council, Jan. 26, 1826, passed the following resolution:—"That the medal be awarded for the most important discoveries or series of investigations completed and made known to the Royal Society in the year preceding the day of the award,"† that is, I presume, St. Andrew's Day, or Nov. 30, of the same year.

In 1826, Jan. 17, Mr. Babbage communicated a paper to the Society, *On a method of expressing by Signs the Action of Machinery*, which was read March 16, and printed in the *Phil. Trans.* for 1826, Part 2. The Council, however, assigned the medals of that year to Dalton and Ivory, the President in his speech giving as a reason "that, though more than one discovery had been announced to the world, yet there were none which could be said to be as yet fairly and securely established." He added, "that the labours which may be said to have acquired their full authority only within a short period" might be considered "within the literal meaning of the foundation.‡

I do not assert that Mr. Babbage considered himself to have had a claim to the royal medal for his memoir in 1826; but from his language I conjecture that he thought so."§

In pages 152 and 153 Mr. Babbage favours us with his views

* I never heard who urged this grant upon Sir Robert, but with all respect to their good intentions, I doubt their wisdom and his.

† This resolution was soon after rescinded.

‡ From the statements at page 139 of *The Exposition of 1851*, it would seem that *Mechanical Notation* has been improving ever since 1826, and is not yet in such a state of completeness as to allow of its being safely published as a well-considered system of signs.

§ In these matters there is a sort of compensation. In 1823 the Astronomical Society gave Mr. Babbage their gold medal for his *Calculating Machine*, which was then scarcely more than an idea. I don't believe there was any jobbing, though the leading members of Council were intimate friends of the medallist; but I think the award was hasty and irregular, and one that does no honour to any one concerned.

in regard to the conduct of Presidents of exalted rank, or who do not permanently reside in London. The President, he says, communicates with, and is led by, some officer of the Society, who misinforms him as to its wishes; and this officer misleads the Council and the Society by misrepresenting the wishes of the President. "Under these circumstances, it is difficult to oppose the expressed wishes of the absent President, and, strangely enough, without any intentional deceit. President, Council, and Society, are supposed to be unanimous in doing what each by itself thinks inexpedient." Strange enough, as the American papers say, *if true*. I thought the Council of the Royal Society, when I belonged to it, rather too compliant with the *expressed* wishes of the Duke of Sussex, the President, *in person*,* and that when I opposed him, perhaps too abruptly, I had not the support which should have been given me; but I never heard of, nor do I believe, such sycophancy as Mr Babbage imagines. If such a case as he supposes should occur, the mistake would surely be found out, and the deceitful medium got rid of.

In page 154, Mr. Babbage—a second mountain in labour—commences his attack upon me in form. He says he had hoped that Mr. Weld's *History of the Royal Society*, and two criticisms on that work in the *Athenæum*, would have rendered any further explanation respecting the *Calculating Machine* unnecessary on his part. If Mr. Babbage had paid ordinary attention to what passes, he must have learned that secondary evidence is never taken when primary is to be had, and that no respect can be paid to any account of his conduct which does not proceed *directly* from himself, and on his *own responsibility*. I believe implicitly that Mr. Weld and Mr. De Morgan have stated correctly everything which came to their knowledge, and I believe that, in his communication with them, Mr. Babbage spoke the strict truth to the best of his belief. But the account is neither complete nor satisfactory, and the public never will be, or ought to be, satisfied, till it has a full, true, particular, and connected history from Mr. Babbage's own pen, with documents. Extravagant expectations have been excited, a large sum of money has been wasted, and the blame must naturally rest

* It was on the subject of the royal medals, which greatly interested him. He came to the Council, and, laying great stress on the King's wishes, proposed certain conditions for their distribution. I said that I would *rather not have the Royal medals at all* than have them on the conditions which he laid down. I don't mention this either as a very bold or very meritorious act, or that I may prove my own personal exemption from Mr. Babbage's charge, but that I may do justice to the Duke. He had been very kind to me, this was his pet project, and my vexation on having to thwart him made my opposition less courteous than it ought to have been; but no impatient word fell from him, nor was his subsequent manner to me at all changed. As to the other Presidents in my time—Davies Gilbert, the Marquis of Northampton, and the Earl of Rosse—I cannot conceive that any one could scruple to speak openly before them, or fancy that they would take offence at an honest opinion (I once brought over the Marquis to my opinion in a debate). The fault I should find, if it be a fault, is, that they were too gentle and too considerate. A little more strictness and severity would have saved the Society both time and trouble not unfrequently.

at Mr. Babbage's door, until he clears himself, which he cannot do by dribbling incomplete revelations through unauthorised channels.

Mr. Babbage's assertion, that "many persons have expressed their suspicions that some occult agency was at work to prejudice the government against him," I shall believe, when I hear who those persons are. If such an idea entered any sane head, Mr. Babbage must himself have put it there. The conduct of the government can be explained, I conceive, on much simpler principles, without looking farther than the proceedings of Mr. Babbage himself.

In the next two pages we are told of Mr. Babbage's attention to the passions and interests of men, of his freedom from all rivalry, of his services as a reformer and politician, &c., all which, to say the least, are disputable, not to say controvertible, points. After this solemn exordium and an illustration from natural history, not particularly probable nor much to the purpose, Mr. Babbage proceeds to guide the young enthusiast by the torch of his own experience. Mr. Babbage has, it seems, been the victim "of a quarrel in which he had no part, and with whose origin he is unacquainted."*

Mr. Babbage states that two-and-thirty years ago Sir James South was "on terms of intimate friendship with almost all the persons at that time eminent in science," among whom Davy and Wollaston are particularised. I conceive that neither of those eminent persons would have liked to be called "intimate friends" of Sir James South; they were, no doubt, familiar acquaintance, and dined with him.† That Mr. Fallows was his guest, for the purpose of using his instruments, is also quite true, though the queer termination of that hospitality is not alluded to. It is *not true* that Sir James South assisted him "in acquiring a practical knowledge of instruments" (beyond the merest rudiments,) for the best of all reasons. This knowledge Mr. Fallows received from Troughton, the common teacher of us all. In return for this hospitality, Mr. Fallows made computations ‡ which Sir James published in several periodicals, but *in his own name*.

At page 157, Mr. Babbage gravely states that "*a party had been formed* adverse to Sir James South" when he was elected President of the Astronomical Society, and that this party elected me as Secretary. It happens, unfortunately, that the very person,

* Rather a misty indication to the young enthusiast how he is to save himself.

† So I suppose, though I don't recollect to have seen either of them there. By the way their names are introduced, one would suppose that they had something to do with the Astronomical Society, to which, however, Davy never belonged, and Wollaston only joined it on his death-bed.

‡ So said Dr. Pearson, and I could confirm him by internal evidence,—I think their friendship cooled before Mr. Fallows' departure, and I know that Mr. Fallows thought himself ill-used by Sir James while he was residing at the Cape, and complained to his friend Troughton.

who most pressed me to be Secretary, proposed Sir James as President, viz. Lieut. Stratford. It is likely enough that the other gentlemen who brought forward Sir James South (Mr. Baily and Captain Beaufort) did so with some scruple, and though they engaged that he should not compromise us by his outbreaks, yet that they were not quite easy about their pledge, and were not sorry to have my aid. Lieut. Stratford, the other Secretary, and at that time on very intimate terms with Sir James, was most anxious to have me for his colleague, and the expression fell from him, he promising to co-operate with me for the same laudable purpose, which he did. So much for Mr. Babbage's mare's-nest : there is not a particle of truth in his suggestion.

I do not remember the conversation between Mr. Babbage and myself at the meeting of the Greenwich Visitors in March 1831, or the words attributed to me by Mr. Babbage at p. 157. There was a meeting, the real object of which was, as I learned afterwards, to give the *Nautical Almanac* to Lieut. Stratford. I remember that I agreed with those who thought Mr. Pond was not able to execute the two offices of Astronomer Royal and Superintendent of the *Nautical Almanac*, but I am pretty sure I added the *caveat* that, in certain cases, the offices would not only be compatible with but ancillary to each other. If Mr. Airy were not present, I mentioned him as a person who could make them so, and I have some faint recollection that Sir J. Lubbock thought so too.*

Whether on that occasion I expressed my determination to put down Sir James or no, I cannot recollect : but after the narrative I have given of the steps taken by me *in that very month to eliminate* him from the Council of the Royal Astronomical Society, it is very probable that I did so express myself, if the occasion offered. It is very probable, too, that I expressed then, or at some other time, my disgust that men of character, like Mr. Babbage and Captain Beaufort, should give Sir James their countenance ; whether I used the words attributed to me by Mr. Babbage then, or at some other time, is of small importance. In persecuting Mr. Pond, and in supporting Sir James South, both Mr. Babbage and Captain Beaufort acted, as it seemed to me, an unbecoming part, and I think so still.

The inference which I suppose Mr. Babbage means to draw, is, that my persecution of him, and perhaps of Captain Beaufort, *commenced* from this date. Now, as to Mr. Babbage, I can only say I voted for him when he was candidate for Finsbury, which must have been two or three years later than this conversation, and though this only proves that I preferred him to his competitors, it

* Yet, on the late vacancy, if the appointment had rested with me, I should have appointed Mr. Adams. I am very well satisfied, notwithstanding, with the choice of the present Superintendent, Mr. Hind, who is perfectly competent, very zealous, and who has sustained the honour of practical astronomy in this country by his numerous discoveries.

is scarcely consistent with any very malignant feeling towards him. To the best of my recollection, we continued to be on friendly terms up to the time of his giving evidence on the trial in 1835. Sir James South did not, during this period, trouble either the Royal Society, or the Astronomical Society, or the Board of Visitors, with his company; so the subject-matter of dispute between Mr. Babbage and me did not offer itself, and we had no other motive for quarrelling. As to Captain Beaufort, I undertook a long and troublesome job for him long after this date (I am afraid I "*volunteered*," the Captain being in a dilemma), viz. to give the Admiralty information respecting Groombridge's Catalogue, which was already printed, but which there was some scruple* in publishing. After these facts I think that any words I may have uttered in my indignation at the *set* made against my friend Pond, by persons who knew nothing of his merits, cannot be considered as of serious import. Whatever I may have said in heat, I have never seriously attacked Admiral Beaufort *at all*, nor Mr. Babbage *til the present time*; for though I made no scruple in talking of his defeat in the matter of the great Equatoreal, I have now for the first time *published* it. The setting him right in a discussion about the planet Neptune can scarcely be styled a putting down.

At page 157 Mr. Babbage says that, in the course of 1832, the large equatoreal mounting "which had been contrived and executed by Troughton for his friend Sir J. South," was found to be "an entire failure," and that this produced "a difference

* The work in this form was *suppressed*, and very deservedly. The preparation and publication of the Catalogue was subsequently undertaken by Mr. Airy, gratuitously, and brought out in a way to do honour to Mr. Groombridge and to the country. But I claim the merit of having pointed out the worthlessness of the Catalogue *as it was originally prepared and printed*, and for having saved the Admiralty from the discredit which it would have brought upon the astronomical science of the country, and for this service I merited the thanks of their lordships, which I received. It is true that in the course of this very troublesome investigation, I thought myself ill used by Captain Beaufort, and it was to that usage I must have alluded in the words cited. page 161. Luckily one does not always carry purposes of this nature into effect, and I am sure that the gallant admiral has had no ground of complaint against me, hitherto. But if he thinks so, and will either say so himself or *commission* Mr. Babbage to say so, I am quite ready to state my ground of dispute with him. "His honest and well-earned fame" certainly "does not need the pen of Mr. Babbage to protect it;" and indeed I doubt whether that pen would succeed in *any defensive* operation. Mr. Babbage had better try it in his own defence, which is the more pressing matter. I have already said that the evidence given by Captain Beaufort on the Equatoreal trial was so fair that he was not cross-examined. It was I who particularly desired that he should be invited to inspect the instrument; and when it was objected that this was a dangerous step, his feelings being so strongly on Sir James's side, I replied that Captain Beaufort was an honourable man, and one who would give true evidence of what he saw, whether he liked it or not; adding, that I had no doubt I could show him how to measure a double star with the instrument. Captain Beaufort declined our invitation, as I have already stated, on the ground that he was Sir James's friend. The truth is, that he must have quarrelled with Sir James if he had accepted our offer, and he had many reasons not to wish this.

between friends who esteemed each other highly.”* At the next page he says he has not “the slightest doubt that this unfortunate affair might easily, by the exertion of judicious friends, have terminated in the entire restoration of their former friendship.” Why did not these judicious friends step in, then, with Mr. Babbage at their head? What might have been done if Sir James South had had “a judicious friend” bold enough to speak plain truth to him, and who knew something of mechanics and astronomy, I can’t pretend to say; but he had no such friend, nor do I think he deserved one. The next assertion, “that I took effectual means to prevent this course,” is a simple falsehood. I never meddled in the matter at all till the quarrel was established; and was indeed so completely engaged in the latter half of 1831 and the first half of 1832 with my duties as a Commissioner for settling the boundaries of parliamentary boroughs, that I knew very little about the matter. I have already stated that the quarrel was fully engaged, and beyond the making up even of such an arch-peacemaker as Mr. Babbage himself, when I interfered; and this is well known to Mr. Airy and Mr. Simms, somewhat better judges of the temper of that high-spirited old man than Mr. Babbage.

I have already mentioned how and why I “volunteered” my services to remedy the faults in the great equatoreal, and to make Sir James South pay his just debts. Sir James’s declaration, “that no one could have been pitched upon more obnoxious” to him than I, may be true enough; but there was no one else, that I have ever heard of (will Mr. Babbage suggest one?), who was able and willing to undertake such a job. If Sir James had been a man of sense and temper, he would have looked on me as a mere workman belonging to the firm (he did so at times, but could not act consistently), and consoled himself with the reflection, that if I succeeded, he might profit by it, and if I failed, he was no worse off than before; while I should, to some extent, be discredited. When Mr. Babbage adds, “that it was at last discovered that no after contrivances or expense could correct the errors of an instrument, itself radically defective in principle,” he says what is false, as I have already proved.† The arbitrator thought differently, and he is a somewhat different authority from Mr. Babbage.

If the correspondence with Sir James was “irritating,” it was

* Knowing Troughton a good deal better than Mr. Babbage did or than Sir James could do, I should qualify this “high esteem” very considerably. Troughton was flattered by Sir James’s *culte*, but he never thought him, *I know*, either a guarded speaker or a liberal man to deal with, or a man of any science; and I have heard him express these opinions in pretty clear language, long before the quarrel.

† Must I again remind Mr. Babbage of his admission, “that he did not know so much of the instrument as those who had spent much more time over it,” and that he really knew nothing whatever? After the instruction he received on his cross-examination, something better might have been expected from him than a repetition of his old *mumpsimus*.

his own fault. No doubt I quizzed his grandiloquence; but when he wrote as became him, I replied as became me. Let him *publish* the correspondence; he *printed* it long ago.

I have no doubt that Mr. Babbage does find it very "curious to observe the vigour and energy with which I applied myself to the exercise of my earlier studies." I did know something of law and a little mechanics and astronomy; and I had temper and courage enough to make successful use of this knowledge. Whether I deserve praise or blame depends on the subject-matter on which these qualities were employed. I am sure that my cause was a righteous one, and who thinks otherwise? I did cross-examine Mr. Savage to save time (Mr. Starkie was absent during the examination in chief), and I succeeded in my cross-examination.*

Mr. Babbage now comes to his *cheval de bataille*, that "I undertook to *intimidate witnesses* on the opposite side." He, no doubt, believes this; but it is a delusion, and rests upon no solid foundation of fact or probability. Let him name the persons intimidated. Mr. Babbage says he was reluctant to be a witness: it is a pity that he overcame this reluctance. He says "that Lord Abinger represented to him that his evidence was essential for the justice of the case," and that on that ground he reluctantly waived his objection to appear. Now what Mr. Babbage ought to have done was, to have declined taking any part *unless Sir James South would first act fairly*, i. e. allow the instrument to be finished. If he told Lord Abinger, or anybody else, that his evidence was important in a cause, of course he would be told that he ought to produce it. I don't suppose Lord Abinger knew anything of equatoreds, or anything about the matter at all, except what he learned from Mr. Babbage. But he was a lawyer and also a lord, and, therefore, Mr. Babbage thinks he was an authority *in morals*. I think, on the contrary, that his opinion was that of one who speaks on imperfect data about a matter of which he is ignorant; I believe that he would have highly lauded my conduct, if I had consulted him about it. The truth I fancy to be, that Mr. Babbage *sought* this employment, having a very erroneous estimate of his powers, and not sorry to exercise them at the expense of persons whom he disliked. Besides this, he had really no choice left him, unless he chose to quarrel with his friend, which, on many accounts, he did not wish. Mr. Babbage has a considerable taint of flunkeyism, and would not willingly exclude himself from a house where he might not unfrequently meet with people of rank. He has also another use

* Some of my friends, who are also Mr. Babbage's friends, think his description of my "vigour and energy" highly laudatory. I think he only does me justice. If I had "volunteered" my services (even at the suggestion of a noble lord), and then upset my client, and disgraced myself by my profound ignorance of the commonest mechanical truths and my incapacity to analyse my own experiments, I should feel very much ashamed of having thrust myself forward.

for Sir James' acquaintance, viz., to give himself a supporter, though but a mean one, in his spiteful attacks.*

I have already given an account of the wrangling between Mr. Babbage and myself, which is more full and far more accurate than that which the reader will find at page 160. I am sure that my threats applied to the cross-examination; in which I intended to expose the mechanical ignorance exhibited in Mr. Babbage's examination in chief, and to the future publication of this ignorance. I intended, undoubtedly, to discredit his evidence, and I think I did so. If I alluded to *another subject*, it was to Mr. Babbage's conduct as Lucasian Professor. But if I had said all that Mr. Babbage puts into my mouth, what then? I suppose that he and his friend Sir James, though the largest and least fair dealers in the practice, have no exclusive privilege "for showing people up." I don't know what weapons Mr. Babbage suspects me of employing, nor do I think his measures for making them inoperative by a whining complaint to the arbitrator were very effective or judicious. He seemed to me like a lubberly school-lad, who, having picked a quarrel, and got thrashed, comes with his finger in his eye to complain to his master. The arbitrator did not see anything in it, except, perhaps, a childish squabble between two grown men. His only remark was,— "that it was agreeable to find, though of course it was to be expected, that two gentlemen agreed in giving the same account of the same conversation." Not one word of blame. He could not have believed, therefore, that I had "undertaken to intimidate witnesses," still less, that I had attempted to frighten Mr. Babbage into "modifying his evidence."

The subsequent paragraph is still more remarkable. "As he had ventured, *after* my having given evidence on oath, to threaten me with injury, with the hope of inducing me to modify that evidence on cross-examination, it appeared to me probable that he might have been tampering with the evidence of other witnesses in the same cause, who, from their position or circumstances in life, might be compelled by the fear of his vengeance to shape their evidence so as to adapt it to his views." I own that if I had threatened Mr. Babbage *before* he had given his evidence, there might have been some shadow of excuse for his insinuation, though I should have thought any one who feared such a threat a very hen-hearted fellow. But *after* examination naturally comes cross-examination, and the *publication* of his blunders formed the substance of my threat. I have, perhaps, no very high opinion of Mr. Babbage's moral courage, but I certainly never rated him so low as to believe he would "modify his evidence" from fear, nor do I believe that such an imagination can issue from a sane brain. I deny any attempt to intimidate any one, and

* It is curious that the "*Thirty-nine Charges*" followed soon after "*The Decline*," and that the letter in the *Mechanics' Magazine* seemed suggested by an account of "*The Exposition of 1851*." It is curious, I mean, if *accidental*.

must trouble Mr. Babbage to mention names if he wishes to escape a very unpleasant designation. Sir James South, indeed, did threaten with his displeasure all those who should examine the equatoreal on our invitation (I have mentioned his behaviour to Mr. Bramah); and he threw in our way all the obstacles he could; which was dishonourable enough, besides being unavailing.

I ask, why should I wish Mr. Babbage to "modify his evidence;" and in what way could it be modified to suit us better than it did? I am unable to suggest any improvement in it for *our* purposes. It suited us to a tittle, and I would not have lost it for a great deal at that time. I will explain this to Mr. Babbage. Before Dr. Robinson's observations, we only knew the qualities of the instrument as a *micrometrical* instrument, and it was possible that some serious errors might have existed without our knowledge. These would have damaged our case, if it were considered that we had ever undertaken to do anything specific,—anything more than our best. The question might easily have come to depend upon this, whether in making a *new* instrument, *under the defendant's constant superintendence and perpetual direction*, we had not a right to be paid, *even in case of failure*, unless *our ignorance was palpable*. We elicited from almost every one of the defendant's witnesses, that he had a scheme of his own for mounting an equatoreal, some of them absurd enough. The general ignorance of the defects and of the causes of the defects of the large equatoreal, were carefully noted as the several witnesses were produced; but in this respect Mr. Babbage was more valuable than a score of common mortals. The blunders of the Lucasian professor, of the inventor of the Calculating Machine, &c., were sufficient to cover ours, if they had been far larger than we knew they could be. If Mr. Babbage did not understand these matters, *with the instrument before him*, how is it possible,—we should have said, had we failed—that *we* could *foresee* them? At that time I considered Mr. Babbage's ignorance as a sort of sheet-anchor on which we might rely, if everything else failed us. Happily there was no need of this line of defence, and Mr. Babbage and his evidence dropped quietly out of the argument as much as if they had never been.*

What right has Mr. Babbage to state in this undoubting manner, what my motives were for threatening him with *exposure* (not *injury*, be it understood)? He did not venture on any such interpretation when he complained to the arbitrator (he would only have got himself laughed at), and I believe it to be an afterthought, to season his accusation. I say that it is an *utter falsehood*, and I suppose I am a better judge of *my* motives than Mr.

* I must, in justice, say that Mr. Babbage's experimental proof of the small bending of the axis, though he did not understand it, in conjunction with my own proof of non-twist, did yeoman's service with a considerable number of witnesses, who had passed muster well enough, after dinner, at Campden Hill. They were Scylla and Charybdis, with this difference, that the poor men fell into both, often without knowing it; for when we felt the arbitrator was satisfied, we stopped.

Babbage can be. I felt great contempt for Mr. Babbage's conduct, and for his mechanical and astronomical ignorance; and I expressed it very openly, and to himself. This is a plain statement which Mr. Babbage's super-subtle understanding cannot comprehend.

Mr. Babbage does not seem to understand that, when you appeal to a man's own evidence against himself upon any point, you must cite *all* that relates to that point. He says I threatened him; yet he takes no notice of what I also said before the arbitrator, "that I would not attack him underhand." I suppose every one will see that this engagement limits the extent of the threat, and confines it to open and avowed proceedings—such, for instance, as this present pamphlet. Mr. Babbage is not possessed of, I suspect he does not comprehend, the chivalrous feeling which makes it a duty not to attack any one secretly; and I do not expect him to believe that, on account of this very quarrel between us, if not for other reasons, I should have abstained most scrupulously from earwiggling any one to the detriment of his Calculating or Analytical Machine. If I had ever spoken on the subject *pro* or *con*—and I am pretty sure I never did—I should have stated at the same time my own ignorance on the subject, and that I was not on good terms with the inventor. But I have always confined myself strictly to one point—the necessity of fuller, and clearer, and better information on the subject.

The passages quoted as mine in page 161, are pulled out of the context; they may be mine or they may not, and I have nothing to remark upon them which I have not said already. In talking to Mr. Babbage, I may have spoken unguardedly, and said more than I should have done, had I been less excited. I did not suppose that I was talking to all the world when I was pouring out my spleen in Mr. Babbage's ears. I do not allow that Mr. Babbage's statement, where it differs from mine, is correct. In his account to the arbitrator of our conversation, he omitted the main point of my rebuke, that he had espoused the cause of a man who, as he himself admitted, had not acted honestly in refusing us permission to finish and prove our work, and who was moreover a quack and charlatan.* The short-hand writer might very easily have taken down Mr. Babbage's speech (perhaps it was written beforehand, for my adversary is not a very ready orator), but if I am to judge by the disconnected sentences attributed to me, I should say that he had failed in reporting me. If I had the whole before me I could not possibly explain what was said and meant, though it is really a matter of no consequence.

* Mr. Babbage, in complaining to the arbitrator, narrated only a portion, and the least material portion, of what I said. I might have set him right in this respect, if it had been worth while, but it was not: the whole was *extraneous to the cause in hand*, and I did not choose to waste any time in debating immaterial issues. I knew that the arbitrator could not be swayed by any such nonsensical whining, and I admitted what I had really said, though it did not give a correct notion of what had passed. The indifference of the arbitrator's remark shows what he thought of the matter.

When Mr. Babbage says, p. 162, "that a decision not satisfactory to either party was given in December 1838," he says that which is virtually untrue. Sir James was condemned to pay the whole of the bill, including the expense of the "after contrivances," which were applied under a special agreement that they were only to be paid for if successful. We got everything except our costs of the arbitration, and came out of the affair "with clean faces."

At page 162, there is a sort of insinuation that the Astronomer Royal and I persecuted Sir James through the press. As far as I remember, I only replied to his attacks; and Mr. Airy only noticed him when assailed by Sir James in the matter of the "Newcastle Lad," and when, years afterwards, the easiness of Sir Robert Inglis had given some weight to some of Sir James's ignorant remarks. Mr. Babbage must know this, and cannot be excused for his misrepresentation.

Sir James was in the habit of writing and printing insolent letters and making anonymous attacks on respectable people, and am I to be made accountable for the disgust which he excited? If, when the ass's bray frightened the beasts, one better acquainted with the origin of the sound had lifted up a corner of the lion's skin and exhibited the long ears beneath, I suppose Neddy would have found himself in a "difficulty;" but his friends, *unless they were very near relations*, would lay the blame on his own meddling and mischievous disposition. I always knew to what genus Sir James belonged, and when I found him as mischievous and troublesome as a real beast of prey, I plucked aside a little of the hide in which he was disguised.

It was from Mr. Babbage's own book, or rather from Mr. Airy's account of it, I *first* heard that I was *supposed* to have meddled about the Calculating Machine, and I heard it with unfeigned surprise. As to my *leading* Mr. Airy, the notion could only come into the head of one who knows neither of us, or who, like Mr. Babbage, knows nothing of character.*

If Mr. Babbage's "curious, though painful study," has produced no more accurate results than those with which he has favoured us in pp. 162, *et seq.*, it is a pity he gave himself so much labour in vain.

I never heard of "any system of disparagement" against any man of science "who refused to give up his ancient social relations with

* A person better acquainted with Mr. Airy and myself than Mr. Babbage is, but who, being one of the unlearned, is not able to see why I defer to the Astronomer Royal so readily and implicitly in his own department, said, that "I acted the faithful Perch to his Dombey." I don't quite allow the truth of this comparison, but it has some appearance. We know each other very well, and I believe have implicit confidence in each other as men of integrity and veracity. But though we agree on very many points, I can't think my respect for him has anything servile or sneaking. As to Mr. Airy, he is the best public servant I know — able, conscientious, and indefatigable; willing enough to listen to reasonable argument, but much more like a "*nasus aheneus*" than "a nose of wax."

Sir James South." I never heard that any man of science had been asked to give up his acquaintance, and I am sure I never asked any one to do so. I never had any "party," I was not the "organ of any party," nor was there ever any "party," so far as I know, "whose avowed object it was to discredit and put down every (or any) respectable person who supported Sir J. South."

In the "melancholy" picture which Mr. Babbage draws of the effect of this "party" (which never existed and whose members are unknown), I recognise no feature of truth or of probability. I know some men whose moral courage is less developed than their other excellent qualities, but I am happy to say that I do not know *one* man such a pitiful coward as to be "intimidated into silence" by the means Mr. Babbage conjures up, or such a fool as to believe in the possibility of such menaces being carried into effect. That men of science should gradually have dropped Sir James's acquaintance is intelligible enough. As his boldness increased, his ignorance became more conspicuous, and his ill-nature, insolence, and love of backbiting, less tolerable.* Men who respected themselves could not be very long in finding out that it did not become them to be made parties to his outbreaks. "One after another almost all Sir James South's old friends and acquaintances amongst men of science have been," I dare say, "alienated from him;" but it has been by his own offensive conduct. I never used any means, or persuasion, or intimidation, to bring this about, though I knew Sir James had tried to induce Mr. Baily and Lieutenant Stratford to give up my acquaintance. The nearest approach to this kind of interference (which I hold to be gross impertinence, and one no gentleman would offer or tolerate) I ever made, was in sometimes warning strangers not to believe too readily what Sir James South said of me, and when I advised — not to buy a telescope of him, because, in Troughton's words, "he was a screw," and sold dear pennyworths.

Of the men who were "alarmed about their astronomical inaccuracies or their mathematics," of "the timid who feared the anger of the dominant party," the "young who dreaded spoiling their prospects," and the old who loved their repose, and "sided with the most numerous party," I can only say that they are the creations of Mr. Babbage's heated brain, and have no more real existence than Don Quixote's giants. I ask for the name of any one individual of these classes; *I don't know one*. As to the "numerous party," of whom I am dubbed captain, I must say that I am also lieutenant, serjeant, corporal, rank and file, and drummer, for be-

* People who remember me at Blackman Street, or elsewhere, in company with Sir James, will bear me witness that I never performed koodoo to him. A man of violent temper and unsubdued manners is frequently allowed great license (especially when he is your Amphitryon) for the sake of peace. In my younger days I did not object to a war of words, and if Sir James had had the luck to have more such acquaintance, *and could have kept them*, he would have been in far better odour than, according to Mr. Babbage, he now is.

sides myself, I can name no one. Every one who knows me, knows this as well as I.

Mr. Babbage considers it "obvious to all who have observed society, that such a system of '*discrediting*' carried on for years, especially against one too much occupied, or too proud to expose it, must end in establishing the set of opinions propagated by the party." I can only pretend to know the society I have lived in, and in that society I am *certain* that no such effect would follow, unless the "*discrediting*" were based on truth. It is not suggested, at least not directly, that any attempts have been made "by the party" to carry on their nefarious persecutions *through the press*, an organ which might mislead the ignorant and unwary, who are numerous enough. "The party" has not been pulled up for anonymous slanders, nor has it vended its libels and falsehoods under deceptive titles, nor expanded them into fallacious generalities, which require some slight knowledge of men and things for their detection. The charge that I have had such a baneful and powerful influence over men of science, and over society, would lead me to suspect a *mystification*, if I were not aware that fun forms no ingredient of Mr. Babbage's character. But the *Diplomate* himself, in Scribe's vaudeville, is not more innocent of all intrigue than I.

It seems the secret workings of "the party" have "misled the various administrations with whom decisions relative to the Difference Engine rested," and, Mr. Babbage opines, that such a result was almost inevitable, unless the ministers "had been highly skilled in mathematical science, or deeply read in human nature." Mr. Babbage and I agree for once "that the former qualification is unnecessary." He thinks "the latter indispensable for a statesman," while I deem it only very desirable, *i.e.*, as an accompaniment to good sense, honesty, and resolution. Unluckily, only one prime minister out of eight, the Duke of Wellington, chose to give a specimen of his skill in reading Mr. Babbage's character, and as he recommended that a grant of 3000*l.* should be made, it might be assumed that his reading was favourable. It is doing more justice to the Duke of Wellington to think that he was guided by the very strong representation conveyed to him by the Duke of Somerset, Lord Ashley, Dr. Fitton, Mr. Baily, Sir John Herschel, and others, (some of whom were probably considered by him as sufficient guarantees,) rather than by any pretence to deep reading of human nature.*

* A college contemporary of Mr. Babbage and mine, who was certainly not favoured by the graces, though an excellent scholar and teacher, made numerous applications for grammar-schools, but always unsuccessfully. Instead of relying on his testimonials, he *would* go and show himself personally to the electors, trusting, I suppose, to "their reading in human nature." Whether Mr. Babbage would have had the same fate as honest — I can't say, but sure I am he did not take the best means to obtain his ends. His vanity led him to wish for interviews, in which he must inevitably have bored the unfortunate premier, without the possibility of enlightening him (Mr. Babbage is not very lucid in his

I do not understand the first paragraph at page 165. Mr. Babbage says, "It is always difficult to trace intriguers up to a direct intercourse with Government. In the present case, the vanity of some of them overcame their judgment, and they gave themselves out as advisers of the Government on scientific subjects. To these I shall not at present refer, but confine myself to citing from official documents two cases of direct communication with the Government by persons on whose judgment it appears to have relied." Mr. Babbage then proceeds to instance me as one "in whose devotion to their interests the Whigs had great confidence," since they appointed me a Boundary Commissioner and a member of the Standard Measure Commission,* and the Astronomer

explanations). When our common friend Lieutenant Drummond was private secretary to Lord Althorp, then Chancellor of the Exchequer, I called on him to beg that he would get leave to admit the object-glass of the Northumberland Equatorial free of duty. He expressed the greatest willingness, saying that it was a pleasure to perform such reasonable requests. He then turned the conversation on Mr. Babbage, who might, he said, have what money he chose to apply for; but, he added, nothing I believe will satisfy him, unless the Cabinet Ministers go in a body to visit the Machine. Lieut. Drummond probably spoke in a little pet, for he was a great ally of Mr. Babbage; but I am pretty sure that he only gave a ludicrous view of what was true in the main, and that Mr. Babbage's continual efforts to bring himself personally into contact with great people was injurious to him. The simple man of science would have encountered far fewer difficulties, and with a little common sense and perception, no difficulties at all.

* Mr. Babbage says, "The Whigs seemed to have had great confidence in the devotion of the Rev. R. Sheepshanks to their interests, since they took the extraordinary step of appointing him, although a clergyman, one of the Boundary Commissioners under the Reform-bill; and he is, I believe, at present one of the Standard-Measure Commissioners." The Whigs (if any of the lords or gentlemen meant by Mr. Babbage read his book) must be surprised to find that they have bestowed their confidence on a person utterly unknown to them, or only remembered for a *bizarre* name. I was asked by a personal friend, Lieut. Drummond, to act as a Boundary Commissioner, and being myself a very strong reformer, I thought it would be cowardice to refuse, as I had at that time no duties, either in College, or elsewhere. The only compensation held out to the Commissioners was, that their travelling expenses were to be paid. So long as I travelled with my very dear and Tory friend, the late Mr. Tallents (as Mr. Tallents was the confidential agent of the late Duke of Newcastle, he was scarcely "devoted" to Whig interests), he sent in the amount of our joint expenses, which were paid; but when I travelled by myself, I did so at my own charge, thinking that I should have spent the same money with less satisfaction if I had been travelling for my amusement. I was engaged pretty constantly for several months, with an interval of rather serious sickness, and the only reward I ever got or expected was one frank from Lord Althorp. Not but that I was richly repaid. I had had my share—a very little one, to be sure—in that great *Conservative* measure, about which I felt as much interest as Lord John Russell himself. I had the satisfaction of finding, that while I was brought into contact with persons of very different politics and in very different stations, I never gave any offence, to the best of my belief, or received any, to my certain knowledge. I have no reason to think that any deception was practised on me, or any information refused me; and my only difference with my excellent colleague was, that his anxiety to do his duty endangered his delicate health, which I knew was of vast importance to his family and others. I obtained besides, from my intimate acquaintance with the details of that measure, the full conviction of the perfect fairness of the principal Reform ministers, Lord Grey, Lord Althorp, and Lord John Russell. In no one instance, to my know-

Royal as another. I suppose, therefore, that we are the "two cases." As a Boundary Commissioner I might undoubtedly have communicated with Lord John Russell *upon boundary subjects*, during the existence of the Commission, that is to say, twenty-two years ago, or three years before my squabble with Mr. Babbage. But, odd as it may seem to MM. Babbage and South, I never asked for an interview with Lord John, as I had no difficulties which were beyond the solution of Lieutenant Drummond. My respect for my political chief did not induce me to thrust myself on his attention, or to take up his time unnecessarily. If I had been as much of a flunkey as my antagonists, perhaps I should have acted differently; but even then I should have kept to my business, and not introduced other subjects. As one of the Standard-Measure Commission, I have not communicated with any higher authority than Mr. Airy, nor have I had the least wish to do so. I have no reason to suppose that Government relied upon my judgment in any other matter; perhaps, if I thought it worth while, I could show Mr. Babbage why it was unlikely that I should be applied to; it is certain that I never was.

Mr. Babbage follows up my case with objecting to Mr. Airy that he has been placed by Government on several Commissions, but he does not mention that, with the exception of the Railway Gauge Commission, for which some compensation was made, these

ledge, was any attempt made to influence or overrule the deliberate decisions of the Commissioners. I can say of myself, and I believe it of all my colleagues, that we never considered, probably did not know, the *party* effect of our recommendations; and I learned experimentally that the world in general is far more honourable and high-minded than Souths and Babbages *et hoc genus omne* would try to make us believe. With this conviction, and the belief that I was useful in an important national transaction, I cared little enough for the sneers of Mr. Croker, even at the time. Now, when all the world is on my side, I feel obliged to my blundering adversary for giving me so good an opportunity to glorify myself.

As to the Standard-Measure Commission, I was asked to belong to it by the late J. D. Bethune, who died—an irreparable loss to education and humanity—some time ago in India. He was Sir James South's counsel in the trial about the equatoreal, and a friend to both Mr. Babbage and me. He asked me if I would serve with Mr. Babbage, and my answer was *that I would not*,—that I had never known a useful proposition come out of Mr. Babbage's mouth, and that he always had been, in my experience, a drag, and not a very pleasant one, on all business. After the Commission was appointed, the unanimous opinion of the members marked out Francis Baily as the fittest person to undertake the restoration of the Standard Yard; and Sir Robert Peel offered him a very handsome salary, which he, of course, declined. On Baily's death, which happened before any considerable progress was made, I, as his friend, and as the only disposable member of the Commission, "*volunteered*" my services, also gratuitously, which were accepted. The history of this tedious and laborious work will, I trust, appear elsewhere, and before long. I will only say, that for several years it has formed my occupation when the state of my eyes and my general health enabled me to carry it on satisfactorily; that my extra personal expenses on this account have been something considerable, as may be easily conceived when it is known that I *reside* at Reading, and my *work* is at Somerset House; and that, like Mr. Pickwick and his friends, I "*defray my own expenses.*" I have received all the remuneration I ever expected,—a letter of thanks from the Treasury,—and am perfectly satisfied if I am thought to have done my duty.

Commissions were unpaid ones like my own. That the services of the Astronomer Royal in the disputed question of the gauges were of a most important and valuable nature, few, now that the heat of contest is over, will deny. How far the Government ought to avail themselves of the abilities of their officers, is a matter in which I differ from Mr. Babbage. It is not suggested that any of the prescribed duties of the Astronomer Royal have been neglected. The Visitors of the Royal Observatory have over and over again expressed their admiration of the manner in which that magnificent establishment has been conducted. Owing to his own admirable method, supported by the zeal and talent of Mr. Main, and by all the assistants, the Astronomer Royal can find time to give the most effective assistance to almost every branch of science related to his own, and to an extent which astonishes even me, who know his peculiar talents and habits and industry.*

As to Mr. Babbage's assertion, p. 166, 167, "that Mr. Airy wishes himself to be considered the general referee of the Government in all scientific questions," it is bad reasoning when good reasoning would have better served him. Mr. Airy has said that "the government gave him an additional computer, to make up for the interruption caused by rating so many chronometers, and his

* If Mr. Babbage knew, what he ought to know, that the Royal Observatory was founded chiefly to aid navigation, he would admit that the subject of harbours, whether tidal or of refuge, belonged properly enough to the Astronomer Royal. The observations of the moon are particularly directed to be made for the same reason, and who has pursued the moon with the assiduity of Mr. Airy? Irreproachable as Astronomer Royal, is he to be blamed if he gives his extra time gratuitously to the Government, or to his friends, or to the Royal and Royal Astronomical Societies, or to publishing the unfinished works of deceased astronomers, or to making himself generally useful to science in England? As President of the Standard-Measure Commission, Mr. Airy did all the work of President and Secretary; and, as to my own department, I am proud to say that I consider myself to have worked *under* his direction (teazing him greatly by my scruples and delays), calling upon him whenever I wanted help, and throwing the calculations, &c., upon his broad shoulders. Before me, Colby and Baily did the same thing, and several members of the Board of Visitors have availed themselves of the same almost unerring guidance. To the Astronomer Royal we owe the reduction of all the Greenwich Lunar and Planetary Observations, the publication of *Groombridge's Catalogue* in an honest and correct form, the observations of Fallows and Catton, and, if it should please God to continue his life, we may expect more of those "diversions from their legitimate object," in addition to "the direction of the many arduous duties of the establishment over which he presides." Besides many valuable papers on optical and astronomical subjects, which, perhaps, Mr. Babbage would allow to be not altogether "illegitimate objects," Mr. Airy discovered the cause of the disturbance of the compass in iron ships, determined its laws, and gave easy practical rules for correcting its effects. Before this investigation, an iron ship could scarcely lose sight of land without the most imminent peril. If the Government applies to the Astronomer Royal on scientific matters in his range, it is because there is no better man, I think no one half so good. Mr. Airy's firmness and straightforwardness correspond to his intellectual qualities. He is no jobber, nor could any jobber make use of him. I can only account for Mr. Babbage's objection to Mr. Airy as a Government adviser, on the Irishman's principle, who declined to be tried by God and his country, as knowing too much about it, and asked for a Galway jury.

own employment elsewhere," i. e. on Government business: "that he considers the Royal Observatory not a mere isolated place for astronomical observations, but as a most important part of the scientific institutions of the country;" that, "he has been uniformly supported by the *confidence of the Government*:" that "his energies have not been wholly absorbed in the mere astronomy of the Observatory, as he has given opinions on subjects of railways and other mechanical matters referred to him by Government."* These are all matters of fact, stated for the satisfaction of the Visitors, and with some pardonable self-gratulation in the Astronomer.

They *prove* that Mr. Airy has been treated wisely and considerately by the Government in his capacity of Director of the Royal Observatory, and that he has given his opinions on *some* mechanical matters referred to him. The conclusion which Mr. Babbage was justified in drawing is, that the Calculating Machine *might* be included among "the other mechanical matters;" and on this basis he might have framed a hypothetical structure of more or less probability. But Mr. Babbage, who is not very careful in matters of reasoning, deduces, as his inference, that "the Astronomer Royal wishes himself to be considered the general referee of Government in all scientific questions," which is impertinent as to data propounded, and to the conclusion which was desired. There is no evidence as to Mr. Airy's *wishes*, and his *wishes* have no bearing on the matter.

At the bottom of the page in which Mr. Babbage has stated Mr. Airy's "wish to be considered as the general referee of Government in all scientific subjects," he improves his proposition into the following form: "According to the Astronomer Royal's own statement, he was their adviser on all scientific subjects," which reminds me of the story of the three black crows. I call particular attention to the process by which Mr. Babbage arrives at his conclusion, it is very instructive as to his state of mind and reasoning powers. A man who can get so wide of the mark in a couple of pages, and *print them*, is not a very safe guide.

"He was too warm on picking work to dwell
So faggoted his notions as they fell,
And if they rhymed and rattled all was well."

Sir Thomas More would have advised him to versify his book.

I never heard that the Government *did* consult Mr. Airy about the Calculating Machine *before I learned it from Mr. Babbage's book*; but I say very decidedly that, if the Government did consult Mr. Airy, it acted very wisely; and if he gave any opinion, I think

* I suppose this is what is alluded to in the dark passage, p. 165, that "the vanity of some of them overcame their judgment, and they gave themselves out as advisers of the Government on scientific subjects." Mr. Babbage is no doubt an excellent person for scenting out vanity, though perhaps he does not sufficiently distinguish it from the consciousness of merit; but why does he use the plural number, and so, grammatically at least, join me with Mr. Airy?

it almost certain it was a judicious one. The best authority in this country, except, perhaps, the Astronomer Royal, the late Dr. Thomas Young, thought the money to be laid out on the Calculating Machine would be better employed as a fund for calculation. Seeing that we have got *nothing* for our 17,000*l* but Mr. Babbage's grumbling, I think many people will be of Dr. Thomas Young's opinion, though it earned him the undying hostility of Mr. Babbage. Perhaps this is only another instance of my conspiracy to discredit Mr. Babbage on account of the evidence he gave about the great equatoreal. That Young died before the equatoreal was commenced, and that I only just knew him to speak to, need not stand in the way of Mr. Babbage's fertile imagination when he discovers my next plot.* I had as much to do with Dr. Young's opinion as with any Mr. Airy may have formed; what I say is, that if the two agree, the odds are, they are right.

At page 166 it is said that Mr. Airy "was unable to draw up a memorial to Sir Robert Peel which he had himself proposed, even though it related to an astronomical subject — our Colonial observatories," on account of his labours in the Railway Gauge Commission. This seems to *insinuate* a neglect of duty. Members of the Board of Visitors know perfectly well, though the readers of *The Exposition of 1851* may not, that Mr. Airy, as Astronomer Royal, is not charged with any observatory but Greenwich; and that in drawing up a memorial about Colonial observatories for Sir Robert Peel's information, he was performing a work of supererogation (which Mr. Babbage should style, if he is consistent, "an injudicious diversion of his abilities from their legitimate object") to be omitted or postponed according to his pleasure.

The anomalous state of the Government observatories out of England had frequently been talked over by Mr. Airy and myself, and he was anxious to apply a remedy.† The simplest way appeared to be, that he should take an opportunity of mentioning the matter to Sir Robert Peel, then Premier, who was

* I do not accuse Mr. Babbage of *inventing facts*, like Sir James South or other romancers, but I assert that he cannot comprehend a *simple fact*, or state it, without adding his own wild surmises, as if they were equally certain, and rested on the same foundation. I dare say he believes there was a conspiracy against Sir James South, and against himself, and that I was the head of it; but he cannot, and he does not, point out one particle of evidence to connect any other person with me, or to show that I attempted to influence any other person, *beyond what I myself have freely stated*. All is mere *conjecture* on the most flimsy tissue of what seem to him probabilities. He specifies no one except the Astronomer Royal, and the Astronomer Royal and I flatly contradict him.

† There was no effective *control*, so that at Paramatta the new instruments were never mounted. There was no *encouragement*, as our petty officials don't much care for science, and know nothing about it. I have heard that the building of the Cape Observatory was delayed for years, because Sir John Barrow had put the plan into his desk, and forgotten, or at least neglected it. The chiefs are easy to deal with, but you can't get at them: the underlings, as a rule, are, I ought rather to say *were*, no go.

easily accessible. This was done, and Sir Robert desired to have a written statement of suggestions for remedying the evils. Mr. Airy accordingly drew up a memorial, but after some delay; having been closely employed on the papers of the Railway Gauge Commission, which was surely a more pressing national question.

Mr. Airy recommended that the directors of our foreign observatories should correspond with, and report regularly to, a small named committee (he was to be chairman), whose duty it would be to insist on good work at each establishment, and to bring the requests and wants of the directors to the effectual knowledge of Government. Sir Robert Peel received Mr. Airy's letter on the eve of quitting office, so he forwarded it to the Admiralty, requesting their lordships to lay the matter before the Board of Visitors. This was done; but, unluckily, the clerk, whose business it was to transmit the matter officially, *suppressed* Sir Robert's letter, and *suggested local boards*, — a measure which would have been far more injurious than letting things alone.* The Visitors put this meddling letter aside, and recommended a small committee, such as the Astronomer Royal advised, and under his presidency; instead, however, of naming certain persons, they proposed to appoint the members themselves out of their own body. There was now this difficulty: Mr. Airy refused positively to take any share in any fresh body in which Sir James South could be included, while it was admitted by all, that the committee, without Mr. Airy, would be useless. A considerable number of the Visitors were of opinion that we should apply for fresh warrants, in which Sir James South's name should be omitted, and some steps were taken to procure them; but I think the easy nature of Lord Northampton proved an obstacle.† It was understood, I think, finally, that the Admiralty would, in future, be guided mainly by the advice of the Astronomer Royal as to the Cape and other Observatories, — which was, perhaps, the second best solution of the difficulty.

Mr. Airy's objections to Sir James South were, I presume, principally on the following grounds. When Mr. Airy was preparing Groombridge's Catalogue for publication, he applied to Sir

* Official gentlemen, when they are commissioned by their superiors to ask scientific bodies for information, generally contrive to give a great deal of unnecessary trouble by throwing out ill-advised suggestions, which are treated with more ceremony than they deserve, from respect to their masters. Some years ago the Astronomical Society was consulted as to the propriety of keeping up both the Observatories of the Cape and Paramatta; and the clerk, to show his knowledge, suggested that one must be unnecessary, as they were nearly in the same *latitude*. I wanted to return a saucy answer, that there was another important element, which had apparently escaped attention, viz. the *longitude*. It gives trouble and costs time to refute civilly, but decidedly, such idle suggestions.

† Sir James has not attended a meeting of the Board of Visitors since his reproof by the Duke of Sussex. He did, indeed, come to the visitation in 1853, when he thought I was abroad; and I believe intended to support Mr. Babbage in his attack upon me in my absence. But when he saw that I was there to defend myself, Sir James showed his discretion and retreated.

James South, who had bought Groombridge's Circle, for leave to examine it. The reply was studiously offensive; and when Sir James published it in the "*Times*," as he thought fit to do, a few years afterwards, he added, that he had immediately forwarded it to a military friend of high rank;* in other words, *that he had attempted to provoke Mr. Airy into a duel*; for which he had no grounds except that Mr. Airy had advised us in perfecting the Great Equatoreal, and given a very decisive testimony in its favour.

Upon another and later occasion, Sir James induced Sir Robert Inglis to believe that the Astronomer Royal had treated *Sirius* and *Fomalhaut* with culpable ignorance and neglect. How the good-natured baronet, and the spiteful knight, fared under Mr. Airy's scalpel, any one may see, who will refer to the *Athenæum Journal*, No. 978, July 25, 1846.

At page 167 Mr. Babbage says, "I have now traced the connexion of the Rev. R. Sheepshanks (who had avowed his determination '*to discredit me*' and also to '*attack me on another subject at a future time*') through his friend the Astronomer Royal, with the Government. Mr. Babbage has done no such thing. We are rather at issue about the precise words used by me when we were alone together. I certainly said "I would show him up;" that is, I would expose his blunders about the great equatoreal. If I alluded to any other sore point (which I don't now recollect, but won't deny), it must have been to his discharge of his duties as Lucasian Professor. I am certain, as I have said elsewhere, that I never thought at that time about the Calculating Machine, which might have been progressing prosperously for aught I knew to the contrary. Mr. Airy will confirm me in my assertion that I never tried to influence him against the Calculating Machine; and I don't think he can recollect any conversation between us on the subject (I can't), prior to his informing me of the contents of the twelfth chapter of *The Exposition of 1851*. It would have been rather impertinent in me to suggest my opinion of calculating machines to him if I had entertained any, and I am sure I never did so. I repeat, that I did not know Mr. Airy's opinion of the Calculating Machine *before* the conversation in which he informed me what Mr. Babbage had said of us in his book.

What I have always felt since the acknowledged failure of that unlucky undertaking, and what I have said doubtless to Mr. Airy and other people, is, that Mr. Babbage must be considered as under a cloud until he himself gives a full account of the reason of the failure. I have said that Mr. Babbage owed this explana-

* I believe this was Sir Rufane Donkin, who would scarcely have allowed two civilians to fight on such grounds. Sir James's usual referee was Captain Beaufort, an officer so notoriously brave and so peaceable, that as ——— remarked, one might as well select a police magistrate for a *friend*. But Sir James is no doubt growing wiser. When he had to deal with Captain Grover, he sought the protection of the Queen's Bench, *before it was wanted*.

tion, not merely to his own character, but to the country whose money he has wasted, and to his friends who pressed his schemes on the Government. I have felt this want of explanation, and I think others have felt it too, a "lion in the path" when any application for public money was suggested. I don't blame the Government, who relied on the representations of the Royal Society and of Mr. Babbage's friends; I don't blame those friends (though I am heartily glad I was not one), for they acted with undoubted good faith, though perhaps they were a little carried away by their personal attachment and the inventor's earnestness; I don't even blame Mr. Babbage himself, *for the failure*, he may not be to blame if we knew all the circumstances (I need not say that I have always scouted any charge of pecuniary dishonesty); but I do blame him for his shuffling, indirect conduct, adducing as evidence persons who could only learn their story from him, and giving no sufficient information, on his own authority, of *what* his machine was to do, and *how* it was to do it, and *why* it didn't do it. I believe this feeling of mine is shared by most of Mr. Babbage's friends who can form an opinion on the matter.

It is curious enough that the only occasion I can remember on which I could have been supposed to influence "the powers that be" against the Calculating Machine, occurred two or three years *before* Mr. Babbage gave his evidence on the great Equatoreal, which is the veritable *causa belli*. Not long after the passing of the Reform-bill, I was invited to dinner by a nobleman who had taken a very active part in the bill, and who was pleased to express himself favourably as to the way in which I had executed my share in the work. I was, I think (with the exception of Sidney Smith), the only commoner present. During dinner, a lady asked me, the full length of the table, whether the Calculating Machine would not be of great service in the computation of the *Nautical Almanac*. My reply was, that I did not think it would; that its design was, I believed, to compute tables, by which it was supposed the calculation of the *Nautical Almanac* and of other similar works would be facilitated. My querist was a countess, and there were, perhaps, three or four cabinet ministers present. After this full confession, I have nothing to add on the subject.* Mr. Babbage has traced nothing, for there was nothing

* I remember that one of Mr. Babbage's friends was displeased with my little reply to his attack on the Astronomical Society and its President in the matter of Neptune. I was told it would do mischief, though I did not learn in what way. Surely Mr. Babbage has no prescription to write criminatory letters in the "*Times*," without being answered. I did indeed suggest that he might employ his time better in giving us some account of the Calculating Machine and its failure, than in writing on matters which he did not understand; but in this suggestion I was really his best adviser. The chapter in Mr. Weld's *History of the Royal Society*, and the criticisms in the *Athenæum*, appeared subsequently to my appeal; and if they were in consequence of it, I did Mr. Babbage a service. Let me hope that the stronger and more pointed requisition, which I now make, may be proportionably effective, and that we may at least get an intelligible history of this affair, if we get nothing else.

to trace; and I declare, on my honour, that his idea is a mere delusion.

What Mr. Babbage says, at page 168, respecting Mr. Airy's opinions about Calculating Machines, &c., does not concern me; it is purely hypothetical, for we have hitherto only conjectures and reasonings to show that Mr. Airy was consulted, or gave any opinion at all. Nor, if he did give an opinion, does it follow that Mr. Babbage has any claim, as a matter of justice, to have a privileged communication published. Mr. Babbage's statements to Government, *proving* "the practical utility of the Calculating Machines, and the possibility of constructing them," are in no authentic or public shape; and he must prove his own case, before he calls on any supposed antagonist. A little lower down, Mr. Babbage says, that after he had executed a small model, "he undertook, at the wish of the Government, to construct for them an engine on a much larger scale, which should print its results." I believe this to be incorrect. The truth appears to me to be, that Mr. Babbage applied to Government for pecuniary assistance, on the strength of a recommendation from the Council of the Royal Society, and obtained it, *in consequence of* that recommendation. On referring to page 256, it will be seen, that Mr. Babbage and the Chancellor of the Exchequer had "contrary impressions" as to the purport of their conversation; but there is no ground for supposing that there was any "wish" on the part of Government to engage Mr. Babbage in the undertaking. Money was advanced to him "to enable him to bring his invention to perfection in the manner recommended" by the Council of the Royal Society. This is by no means an uncommon course. Schemes are proposed to Government, and, when *reasonable, intelligible, and strongly recommended*, money is sometimes granted to carry them into effect; but my impression is, that in all such cases, *without there is a specific bargain*, there is no obligation on the side of Government, though there is one on the side of the projector whose whim is forwarded at the national expense. There is surely a vast difference between being engaged by any one to do something for *his* benefit, and at *his* request; and the setting yourself to work upon a pet scheme of *your own*, at his cost, and on your own application.

Suppose an impetuous desire for a Calculating Machine had fallen upon Mr. Robinson, then Chancellor of the Exchequer, and that he had pressed the commission on Mr. Babbage, as the only person who could execute it. Suppose that, while Mr. Babbage was so engaged, Mr. Robinson had interfered and meddled with him, urged matters on, and postponed his other business to devote his unceasing attention to the machine, watching it step by step, until it had arrived at its growth. Let us suppose still further, that the machine, having been hurried out of Mr. Babbage's hands, was let fall by strangers called in to set it up, and that, owing to this, and other faults of haste or mistake, the mechanism did not act with precision. To complete the climax, let us suppose that all payment is now refused, (not one shilling having been advanced to

Mr. Babbage or to the workmen paid by him,) that access is denied, and that the offer to remedy the imperfections gratuitously, is rejected. If Mr. Babbage had suffered any such injustice, no one would have exclaimed more loudly than I at such a mixture of meanness and tyranny; yet this is a precise statement of the treatment which Troughton sustained from Sir James South, and which Mr. Babbage did his best to defend. In Mr. Babbage's real case, I conceive the Government was at liberty to break off whenever it found the connexion onerous, upon paying the expenses incurred, just as Sir James might have done; though it would have been more handsome to continue the work, so long as there was a reasonable chance of success at a reasonable cost.

In 1834 the work came to a stand.* Mr. Babbage coolly says, "Circumstances over which I had no control then caused the work to be suspended." Now, these circumstances are precisely what we wish to have made clear, and also that they did not fall within Mr. Babbage's control.

There was a quarrel with his engineer, Mr. Clement, I know; but though I have heard causes alleged, I do not know where the blame really lies. Clement objected to remove to Dorset Street, where Mr. Babbage wished to transport him, he having already established a business in Lambeth. I have heard that he was not a good-tempered man, and that he was in very bad health: but I never heard anything positively unfavourable, except the "curious anecdote" related at p. 257, which, if it be true, proves something more than the "great perfection to which he was in the habit of bringing machinery." This quarrel, however, was a most unlucky one for Mr. Babbage, not merely on account of the delay and loss thereby occasioned, but in depriving him of the person on whose mechanical skill and contrivance he had hitherto depended. It is to this quarrel I mainly attribute the failure of the Difference Machine;† and it is doubted by some competent persons whether the machine could have been constructed without Clement's aid.

And now the question between Mr. Babbage and the Government assumed a new phase. During the dispute with Clement, Mr. Babbage, unfortunately as I think, conceived the idea of a new Calculating Machine, which he calls an Analytical Machine; and he was ill-judged enough to press the consideration of this *new* machine upon the members of Government, who were already sick of the *old* one, and who could have no notion about the merits of either. For eight or ten years, ministers had been told of the extraordinary powers and utility of the Calculating Machine, and

* Mr. Babbage's examination in the matter of the Great Equatoreal, from which our quarrel is to be dated, was before the vacation of 1835; the cross-examination was after the vacation, that is, *a year after the stoppage of the machine*, so that I have the same defence as the lamb had against the wolf, if I wanted it.

† As I did not know Mr. Clement even by sight, I suppose I am not suspected of bringing about this squabble between two ill-natured men.

they had responded liberally to repeated demands for money to further its execution. They are then told by *the projector himself* that it is doubtful whether this much-vaunted and expensive work is worth finishing, for that he has now hit upon something every way superior; and that he wishes for *their decision* between the two,—the *ignotum* and the *ignotius*—and for *their directions* how he is to proceed.* This *gaucherie* is so inconceivable, that I hope Mr. Babbage will publish his letter to the Duke of Wellington in which these or similar views are stated. From that time forward Mr. Babbage seems to have been unable to get right, or to take a step to make matters clear; and yet, to ordinary understandings, his course was straight enough.

Having undertaken to make a *definite* machine, it was his business to complete it with as little delay, and at as little cost, as might be. Supposing that in the main he was rather to be pitied than blamed for his quarrel with Clement, still he was bound to remedy the loss, which had been caused by his carelessness or ignorance, in the best way he could. I do not understand that any steps were taken in this direction, and the excuse is not sufficient. It is nonsense to talk of consulting a prime minister about the *kind* of Calculating Machine that he wants. He *wants* no Calculating Machine at all; and it is altering the whole state of the question to suppose that he does, or that he is to take any responsible step, or to decide anything about it on his own “reading in human nature.” If Mr. Babbage really felt any scruple about going on with the Difference Machine, he should have proceeded as he did originally; he should have submitted his views to persons who could have understood them, and then made a definite proposal to the Government backed by their recommendation. An ass between two bundles of hay would be far less divided in mind than a premier between two machines,—one not made, the other not begun, and neither understood.

In 1838 Mr. Babbage asked “whether the Government required him to superintend the completion of the Difference Engine,” and to this application—and others of the same nature—he seems to have got no answer till 1842, when he was told that the design was abandoned, and the machine, so far as it was constructed, placed at his disposal.† Whether this decision was a wise one or not, Mr. Babbage gives us no means of judging. We have no estimates of the probable expense of completion. It seems to me

* I believe Mr. Babbage took the opportunity of notifying these opinions to the Duke of Wellington when he was *sole* minister, and holding a score of portfolios until Sir Robert Peel returned from Italy. So far as I remember Mr. Babbage’s account (we were then on amicable terms, and dining near each other in Trinity College), the letter to the Duke was of *considerable length*, and made *no definite proposal*, but suggested his willingness to construct the new machine at their request, &c. &c.

† This was in 1842, in Sir Robert Peel’s administration, when Mr. Goulburn was Chancellor of the Exchequer; so that I cannot have brought my supposed influence with the Whigs to bear on Mr. Babbage. I fancy that I have as much influence with one party as the other, that is to say, just nothing.

that Mr. Babbage ought to have *offered* to complete the Difference Machine, since its construction was originally *urged* by himself, and not have contented himself with merely asking whether he was *required* to superintend it. This is a departure from the original terms; for there is no evidence that he was ever *required* to commence the machine at all. He was *allowed* to do so at the public expense, and it was his duty to complete his work, unless Government refused his *application* for the necessary supplies. After the reply of 1842, both parties are undoubtedly free; but the previous circumstances require a continuous history, with documents in full.

Those who know how self-opinionated and wrong-headed Mr. Babbage is, and always has been, will have no difficulty in conceiving that his applications to Government must have been troublesome;* and that as his objects were unintelligible, and his claims on their time and attention very importunate, he must have been considered *a bore* by both ministers and secretaries. Add to this, that he came frequently for money, which is an abomination in all public offices, and that the "friction" in such transactions, if there is the least departure from routine, is immense; and there will be no need of evoking me or my "party," or any "intrigues" whatever, to account for the difficulties which Mr. Babbage met with connected with the Calculating Machine.

At page 169, we are told "that there are several offices in the appointment of Government, for which Mr. Babbage is qualified, and to which, under the circumstances, he had some claim;" that "every application was unsuccessful, and that whatever may have been the reasons, the conduct of Government has been exactly that which might have been expected, had they been the *allies* or the *dupes* of the party which thought it necessary, from enmity to Sir James South, to 'discredit' the author of the Analytical Engine." To the insinuation in the latter part of the above quotation I have already replied at more length than I fear will find patient readers. I have already challenged Mr. Babbage to name the members of this "party," which, I say, has no existence; and, I may remark, that as ministers have varied, it is odd that *all* should have been *allies* or *dupes* of the same person. I never even heard, till I read Mr. Babbage's book, that he had applied for any Government office. But I have no scruple in saying that I don't know any Government office or any other office for which he is fit, certainly none which requires sense and good temper. If, indeed, we had a quarrel to establish anywhere, I could recommend MM. Babbage and South as very proper representatives of the nation, and men perfectly sure to get us into a difficulty at the shortest notice. I don't myself appreciate very highly sacrifices

* Of my own knowledge I can say nothing more than what I have already mentioned as coming from Lieutenant Drummond; but if he—himself a man of science, a friend of Mr. Babbage, and an expounder of the machine—was annoyed, what must have been the case with ordinary *secs.* and *subs.*?

which were never asked for, and which have produced nothing; but if others do, pray let these sacrifices be compensated in money, and not by filling an office with an unfit person.

In the two following pages, which conclude the chapter, Mr. Babbage waxes very indignant at the calumny, that "he had received a large pecuniary reward for his services." A calumny it is, as all well-informed people know, and one which is quite inconsistent with Mr. Babbage's character. But though I am, and always have been, convinced of his purity in money matters, I think that the mystery in which the history of the Calculating Machine has been studiously enveloped, has been the main cause of the propagation of the falsehood; and that persons who take their views of men of science from Mr. Babbage's sketches, are hardly to be blamed, if they think all evil of every member of the race. I believe Mr. Babbage *means* to speak the truth; but I am sure that almost every other sentence in his twelfth chapter contains a falsehood in fact, or in deduction; this, I think, I have shown to the satisfaction of any competent person who thinks it worth while to attend to what I have said.

Having disposed of the charges made against me in the lump in Chapter XII., I may be allowed to notice a few passages in the Preface, as being more likely to be read than the rest of the volume, and not irrelevant to the matters in hand.

The Great Exhibition was not intended for "the world's great bazaar;" and the greater number of the English exhibitors would have refused, I believe, to affix prices.* The basis of *competition* could not be adopted even if it had been desirable.

The praise of the United States, which is to be found in the sixth and seventh pages, is too fulsome and too foolish to pass with any well-educated American. If our Transatlantic brethren do not partake of our *prejudices* they have their own, which still more disqualify their judgment. In most cases, an intelligent native *must* be a better and safer authority than an equally intelligent foreigner; and I think that an Englishman who reads an American's remarks on England (or *versâ vice*) seldom has any doubt on the matter.

Mr. Babbage's phrase of "Proud of the only ancestry which is not contemptible," which he applies to the American, is, first of all, not *true*; for good ancestry is not contemptible, nor ever was, nor ever will be: neither is it applicable. Many of the most intellectual and most highly-educated gentlemen of America look with no small satisfaction to their lineage derived from the purest blood of England.

* I agree with Mr. Babbage in wishing to have everything marked with its price, and object exceedingly to the trouble one gives and receives in asking questions. The "*principle of least action*," should govern our conduct in these and similar matters. But the feelings of respectable traders run counter, and I suppose those of the majority of customers; and it is admitted that *each man is the best judge of his own wants and of his own interests*—so we, the smaller party, must be satisfied.

That Americans, because they know us less intimately, can, "as it were, anticipate for us the decision of posterity upon the reputation of those English writers who have never visited her shores," is stark nonsense. That kind of education which is required to form a correct judgment in literary or scientific matters is not more extensive or wider spread in America than England. When I admit that men equally well educated in each country are almost as competent to decide on the merits of their rivals as on those of their countrymen, I fancy I am much nearer the mark than Mr. Babbage; and that sensible Americans claim no more.

Seeing what the objects of Mr. Babbage's cravings are—titles, pensions, stars, red ribbons, institutes—I marvel at this burst of enthusiasm for the great Republic, which has none of these things. Is there any hope that some of those dollars, which seem to distress the States by their accumulation, may be diverted to the construction of an Analytical Machine? or must we say of Mr. Babbage as Horace said of certain grumblers in his time?

"Nostra sed impugnat, nos nostraque *lividus* odit."

He does not love America, but he is spiteful towards England, just as he is towards the Royal Society; and shows it by praising a foreign country and a foreign institution with little discretion.

Mr. Babbage makes this query at page viii. "Who else *could* have fully known—who else *would* have fully told their history?" viz. of his Calculating Machines. I know that he has *not* told us anything about the matter in this book—I mean anything explanatory or satisfactory. There are, indeed, cock-and-bull stories about imaginary persecutions, &c., the main part of which I *know* to be *untrue*. The only history revealed in his book is by other persons, and had been published previously elsewhere.

Mr. Babbage goes on to say, "The facts stated in the following pages are not drawn from any violation of the confidence of private society. Those whose names are mentioned are paid by the nation, and therefore responsible to their employers. Against them I have no personal feelings; their official acts are necessarily mentioned as parts of the system to which they belong."

That Mr. Babbage has been *personal* in his remarks, is evident enough, and I don't object; though I am one of the "illustrations." His freedom has unlocked my tongue, and justified *my* personality, which I feel to be a great comfort. What I complain of is, his vagueness and *impersonality*, talking continually of a "party," but naming no one. I affirm that there is no "party," and never was any "party;" and I ask for names, that I may show the equal ignorance of my supposed co-mates and fellows in conspiracy.*

In this very misty writer one is never quite certain what he means, and I should be loath to defend myself where I am not attacked; but I think the first sentence must relate to the use

* Unless Mr. Babbage takes a leaf out of the book of his shuffling friend Sir James, and *quotes dead men*, I shall have no difficulty.

made by the author of my private conversations. I assure him I acquit him of all blame on this score. If he had confined himself to a simple narration of what really passed, and had not, by omitting the essential parts and interweaving his own surmises with the rest, given a totally false complexion to the whole, I should have had nothing to object to. I must, however, point out to him *one* mistake. He seems to think that, because he introduced an account of this conversation into a trial, that *therefore* it has justly become public property. The question is, whether he had a right to bring it forward on the trial, which, if it had any bearing, I should not deny. I assert that it had not; even the garbled portion adduced by Mr. Babbage made no impression on the arbitrator. What he thought of the complainant's nerve and sense I can only conjecture. To me they seemed on a par with his mechanics.

As my "name is mentioned," I suppose I am included among those "paid by the nation." Will Mr. Babbage mention a single instance in which I have ever received a half-penny for my trifling services either from the nation or from any one else? My work has always been gratuitous, and in almost every instance a cause of extra expense. Will MM. Babbage and South *pay* the bills, if I produce them, of costs out of pocket upon divers occasions? — the pendulum experiments in Cornwall, the determination of the longitudes of Valentia, &c., the publication of the Monthly Notices of the Royal Astronomical Society, the Boundary Commission, and the Standard Measure Commission,* or the Board of Greenwich Visitors.

Mr. Babbage's rule, therefore, if it were true in its most extended sense, does not apply to me.†

* I know that this item has added considerably to my expenses for the last eight years; but it is voluntary, and I am quite satisfied, though I shall not be sorry when I am released from it. Connected with the Board I will give an instance of Mr. Babbage's delicacy, and how carefully he abstains from meddling with matters which do not concern him. As many of the Visitors reside at a distance, it was considered reasonable that a small allowance should be made to non-residents for their expenses. Though I am in the non-resident list, I have always declined to accept anything, because it is against my rule, and because, in fact, the expense is not extra, half my time being necessarily spent in London upon the Standard Commission. Mr. Babbage on one occasion had the singular bad taste to remark upon my refusal, as if I thereby implied some censure upon my colleagues. What *he* might imply by such conduct, is, I trust, no rule by which I am to be judged.

† And yet I can no more claim to be irresponsible than Mr. Babbage himself. We have both *spent* public money, and both are *bound* to give a *satisfactory* account to the Government, and to *intelligent* people who want to learn the truth. I have undertaken, for instance, to restore the standard of length, and to prepare a number of accurate copies. The work has been far longer in hand than I like, and has been more expensive than I expected. My justification is, that I have done my best, and that the delay has not been without its advantages. Messrs. Troughton and Simms' bills are paid by the Treasury on the authority of the Astronomer Royal, who requires me to certify my belief that they are correct and reasonable. I have no means of forming a very accurate judgment, but from the experience of thirty years, I have the same confidence in Mr. Simms' integrity that I have in Mr. Airy's, and that I believe both have in mine. The time for giving an account of my operations is not yet come; when it does, I will tell my tale out of face, and not in Mr. Babbage's fashion.

Another person *named*, and therefore included in Mr. Babbage's category, is the Astronomer Royal. Let me first set him right as to his principle, which, when correctly enunciated, is this,—Every one paid by the nation to discharge certain duties is responsible to the nation for the satisfactory execution of those duties. But, except in a figurative language, the *nation* acts, and is understood to act, by its *appointed* ministers, and it is only in their default that indifferent persons can properly interfere, who, in so doing, become themselves responsible for their *competence* and for the *truth* and *propriety* of their judgments. The Astronomer Royal is responsible in his office to the Government acting for the nation. The Government has delegated its office of inspection to the Board of Visitors, who thus are the proper and primary inquisitors of the Royal Observatory, and who are in fault if the establishment is ill-conducted. In matters which don't relate to the Royal Observatory, the Astronomer Royal is as little responsible to the nation as any one else.*

Now Mr. Babbage has no complaint to make against the mode in which the Royal Observatory is carried on,† if he had, the Board of Visitors should be first, and *exclusively*, appealed to. His objections are, that, besides discharging his public and paid

* I dare say this limitation of the rights of interference will appear strange to some of my readers, but a little consideration will show that the irregular meddling which the author's rule would authorise, is incompatible with order or discipline, or any genuine official responsibility. That every official person in the country is to be subjected to the criticism of any spiteful blockhead, anonymous or notorious, for his non-official acts, is *not* for the national advantage or the national honour, neither is it in harmony with our law or institutions; it is merely a bad custom. The great bulk of the people has no time to hear two sides of a question, and is not, and never will be, sufficiently educated to estimate the probable truth of a one-sided statement. Joe Smith, and Mr. Urquhart, and the Editor of the *Morning Advertiser*, have all found believers, though their stories are rather less credible, and not more consistent, than Falstaff's narrative of the "knaves in buckram." When this evil has become a little greater (that is to say, unbearable), I trust plain men will discover that *to lie with impunity* is not a creditable privilege, nor one necessary for Englishmen, be they gentle or simple; and that the man who defames another is bound to show the truth or high probability of his accusation, even though the person assailed be a prince or a minister. In the case of private persons, a preliminary issue would be simple enough, viz., has the public any lawful interest in the matter alleged? A carefully drawn law of libel, administered by men of sense, would be of infinite service, and a safeguard of the liberty and respectability of the press, which suffers greatly in general consideration on account of its Potts, and Slurks, and Divers.

† Every year since Mr. Airy's appointment, the Board of Visitors has had reason to admire the efficiency of the Greenwich establishment, and Mr. Babbage has not, I believe, dissented from the general sentiment, though I don't suppose his approbation has been quite so hearty as that of the rest of us. But by his remark at p. 167 he has misled, I have no doubt, several ignorant persons, and among others the Editor of the *Mechanics' Magazine*. This person, improving on Mr. Babbage's hint, has distinctly charged the Astronomer Royal with neglect of duty, and contrasted him unfavourably with his predecessors. According to this blind leader of the blind, the official duties of Mr. Main are clerical rather than astronomical—he is the chaplain, not the first assistant, of the Astronomer Royal. Verily if the schoolmaster be abroad, he should be brought into close and immediate contact with this "father of asses;" and yet I dare say there are still greater simpletons who believe him.

duty, Mr. Airy has performed other unpaid services, public and private. With these Mr. Babbage has no concern. I certainly am not going to take upon myself the task of defending the Astronomer Royal; he pays no heed to his calumniators, and can do so safely: I want to show that the conduct of my adversary is all of a piece, that he mars everything he meddles with, and is not to be regarded as of the least weight or authority.

As to Mr. Babbage's complaints of injury and injustice I do not understand on what they are founded. He broke down, for some cause or other, in the construction of a Calculating Machine, but the expenses were defrayed by the country *without a lawsuit*. He has since invented another, and he says a better, machine; but after one signal failure, he could scarcely expect to be treated a second time with blind confidence, and yet he has taken no sufficient steps to convince us of the utility or feasibility of his proposals. Upon this new machine he has, he says, expended 20,000*l.*, although I believe not a bit of it has been executed. I am heartily sorry, and wish he had been more prudent; but I must remark that he had no encouragement, but the contrary, to incur such an outlay, or any outlay at all.*

I doubt whether *The Exposition of 1851* will find many interpreters "who will *know* that its author has abstained, or who will *see* that he possesses the power, though not the disposition, to avenge injury." It seems to me that he has done his *bitter best*, to attack persons who have done him no *injury* at all, though they may have given him considerable *offence*. I think that Mr. Babbage wants logic, discernment, and a power of comprehending or stating facts; and that, as an adversary, he is about as much to be feared as Sir James South, or the editor of the *Mechanics' Magazine*, with whom he has had the folly to identify himself. The only plausible solution I can suggest for this voluntary degradation is Miss Flite's—"he is a little M—— you know."

I have already attempted to point out to Mr. Babbage reasons which may have induced various ministries to treat him with reserve, and to explain to him that his hypothesis of secret enemies, intrigues, &c., is no more necessary to account for that reserve, than Tenterden Steeple is wanted to explain the existence of the Goodwin Sands.

Perhaps this may be made more clear, if I state *why*, in my opinion, the Astronomer Royal "has been uniformly supported by the *confidence of the Government*."

First of all, Mr. Airy had established the highest possible character in his college and in his university (the most honest,

* As a political economist, I object to paying for unprofitable speculations merely because they have cost so much to the projectors. "*Every man must judge of his own wants and of his own interests.*" But as this is an example which is not likely to be followed, one should not be inclined to press the rule too strictly. I shall not make an outcry if a ministry should take a favourable view of Mr. Babbage's case.

independent, and intelligent bodies I am acquainted with) before he came into contact with the Government at all. He had been a distinguished senior wrangler, and then fellow and lecturer in Trinity College. The Lucasian Professorship, which had been almost a sinecure since the days of Waring, fell vacant, and he accepted it, though with a considerable loss of emolument. On the excellence, the originality, and the scientific and practical value of his lectures, and on their permanent effect upon the studies of the University, I shall say nothing to gentlemen, most of whom are far better qualified to judge than I, though I attended and profited by them.*

After the death of Professor Woodhouse, Mr. Airy succeeded as Plumian Professor; and under his direction the Cambridge Observatory became the acknowledged model of all English Observatories. At the same time, he continued his lectures and examinations, with small help from Mr. Babbage, who was the new Lucasian. When, therefore, Mr. Pond's resignation was agitated, there was no thought of any other successor; and the offer of Greenwich was made by Lord Melbourne spontaneously. At first, I think, there was a little disposition in some of the lower officials to be supercilious,† but this soon ceased; and the highest authorities learned to respect the resolute but composed bearing of the Astronomer Royal, and to admire his simple and straightforward manner of acting.

When Mr. Airy wants to carry anything into effect by Government assistance, he states, clearly and briefly, why he wants it; what advantages he expects from it; and what is the probable expense: he also engages to direct and superintend the execution, making himself personally responsible, and giving his labour gratis. When he has obtained permission (which is very seldom refused), he arranges everything with extraordinary promptitude and foresight, conquers his difficulties by storm, and presents his results and his accounts in perfect order, before men like Mr. Babbage or myself, would have made up our minds about the preliminaries. Now men in office naturally like persons of this stamp. There is no trouble, no responsibility, no delay, no inquiries in the House; the matter is done, paid for, and published, before the seekers of a grievance can find an opportunity to be heard. This mode of proceeding is better relished by busy statesmen, than recommendations from influential noblemen or fashionable ladies.

In pages xi. and xii., is some writing rather in the manner of Charles Phillips's early orations, the exact meaning of which I cannot divine. I think the first page must be an eulogium on

* There is a curious remnant still left of the impression made by Mr. Airy on the University. The name of Professor sticks to him, though it has long been inappropriate, and merged in the superior title of Astronomer Royal.

† In the days of MM. Croker and Barrow the Admiralty manners were anything but gracious, and the scrubs copied their chiefs, as is the nature of scrubs. "But we have changed all that;" there has been a manifest advance in intelligence and civility since the good old times.

Prince Albert, in connexion with the Great Exhibition. But I do not believe that in his case there were any "deeply-rooted prejudices of the upper classes to overcome," or "the still more formidable, because latent impediments, of party* to be removed." The rank and personal merits of Prince Albert, and the love and duty every one bears to the Queen, really left very little difficulty of *this* kind in his way. The conception of the main idea, and the persevering activity with which it was carried into effect, are worthy of all praise, and have, so far as I know, received it.

The next paragraphs must, I think, allude to Mr. Babbage himself, for I cannot anyhow make them construe with the Prince; nor, in fact, with anything in the world except the author's notion of himself.

Whether Mr. Babbage has learned to "achromatize his own intellectual vision," and "rectify its colour blindness" or not, may be left to the impartial judgment of those who can appreciate the specimens he has given of his unclouded penetration. He certainly sees many things which no one else can see; but whether this faculty is what he imagines it to be, or one which makes him "compact with the lover and poet," I leave to the reader's guess.

Considering the novelty of the Great Exhibition of 1851, and the short time allowed for arrangements, far fewer errors were committed than could have been expected.

In the wonderful structure, which will immortalize its designers and constructors, there was originally a small oversight; but this admitted of an easy remedy.†

Mr. Babbage's objections arise from his forming a wrong notion of the advantages and peculiarities of the Great Exhibition, and from confounding this *extraordinary convention of all nations* with the annual exhibitions so prevalent on the Continent. I doubted all along, and I doubt still, the policy of the system of profuse medal-granting. It was impossible that the awards should be satisfactory; and, in one or two instances, the blundering was

* "Still on my daughter."

Mr. Babbage finds "*party*" everywhere, and the word serves him for an argument or an explanation on all occasions. Philosophers of a certain class use or abuse *electricity* in the same way to explain the phenomena they do not understand.

† The building was abundantly strong, almost superfluously so, in every respect but one. The provision against side-pressure from high winds was not satisfactory. A few iron rods, properly disposed, remedied this defect. It is a curious part of the true history of this new English style (it would be more just to call it Paxton's style) that the gentlemen who pointed out this inadvertence were treated as enemies of the great undertaking; and their cautions, which were attended to by the builders, were scorned as idle prophecies by self-constituted patrons. In the new Crystal Palace at Sydenham, which far surpasses the original building, the braces or Xs to resist lateral strain are introduced throughout, and in the greatest profusion. This practical admission of the earlier omission is quite satisfactory to those who can observe and think.

extreme.* The juries expended a great deal of valuable time, and wasted a good deal of temper unnecessarily. One or two competent reporters in each department were all that was required.

Mr. Babbage has dwelt elsewhere at tiresome length upon the encouragement of science and the rewards of men of science; but the only clear conclusion which I can arrive at is, his opinion that he ought to have a large pension or a valuable place; some hereditary title, baronetcy or peerage; and, above all, the red ribbon of the Bath.†

Mr. Babbage has no doubt as to the reasonableness of these claims; and if they were granted, I fancy his "views of the industry, the science, and the government of England," would "suffer a court-change." Perhaps the number of men of science who must, in justice, be previously pensioned, ennobled, and decorated, has not occurred to him. Perhaps Mr. Babbage will give us a specific account of the services and merits for which the rewards he claims are only a fair equivalent. Is it quite clear that he would find this task a very easy one, or that competent and independent judges would agree with his estimate?

Upon the general question, whether science is sufficiently encouraged and rewarded in England, my feelings would lead me to say no; but it is much easier to say this than to suggest a remedy. Up to a certain point, and for certain subjects, our system of grammar-schools, colleges, and universities, is only faulty in details, and admits of easy, gradual, and indefinite improve-

* It is desirable to place one instance upon record; I have heard of others, though none, I believe, so flagrant.

When my friend Mr. Simms was requested to exhibit, he declined. His great instruments were in their observatories (these are only made to order); his mechanically-dividing machine could not be spared; and he had as many orders on his hands as he could execute. I believe Mr. Dollond declined to exhibit for the same or similar reasons. At almost the last hour, however, he got together such specimens of his skill as he could lay hands on, and sent them, as a mark of devotion to the Queen, and of respect to the Prince Consort. He gave notice at the same time that he was not competing for any prize. This notice was not regarded, and the jury awarded him a gold medal; the collection being considered, I believe, about the best of its kind. From some cause, which has never been explained, the Committee of Chairmen, *who knew nothing of the merits, overruled this decision of the jury who did*, and a second-class medal was sent to him, although he declined *the honour* altogether. The absurdity of allowing the ignorant body to overrule hastily the deliberate judgment of the better instructed, is patent enough. The Chairman ought to have interfered only when the skilled tribunal was undecided, or for some very special cause. Oddly enough, there was not one member of this body who knew anything of the subject, for the Chairman of the Special Jury was attending a meeting at Exeter Hall.

† The Order of the Bath was one of Sir Humphry Davy's objects of desire; and though one rather laughs at such a weakness in so great a man, it was surely ill done of the ministry to refuse it. Davy was a Baronet and President of the Royal Society; and as he asked for it, he ought not to have been denied an honour which his predecessor, Sir Joseph Banks, had received.

ment. But there is no subsequent career; and our powerful geometers and splendid classics look naturally for some mode of living which the professions alone offer. A few men, who feel their call strongly, and who *will* follow the bent of their own talents, satisfied "to wear a whole coat or a ragged one according as the world will buy or neglect their works," are produced occasionally, and upon these England chiefly depends for her exact science.* Something might be done by increasing the stipends of the professors in all our Universities, and adding somewhat to their numbers. In the larger towns of the empire, Chairs might be established; and pensions might be granted more freely, and on a more liberal scale than they now are. But there is no security, nor even a very high probability, that, with the best intentions, a constitutional government like ours could effect these objects. The people in general, and their leaders, the public press, are very imperfectly acquainted with science or with the merits of scientific men; and a minister cannot be expected to be infallible, if he had time to make inquiries, which he has not.†

As to ribbons, and other merely honorary distinctions, if they were *always* judiciously bestowed, they would do some good, and be useful in stimulating *amateurs* in certain departments. But you must give something more solid if you intend to direct the full energies of a highly-educated man to severe studies. The provision need not, perhaps, be large, as it will generally be in aid of other means of living. The real difficulty lies in securing a proper distribution. Public opinion, as it is called, is a miserably insufficient guide. A few pensions might be placed at the disposal of the Royal Society; yet they might give rise to cliques and cabals, from which real science in England is at present almost free. But the practical problem is not a simple one; and I cannot help remarking, that those who are loudest in reproaching the nation with its neglect of science are not precisely men of the

* One of our best mathematicians, Sir John Lubbock, and one of our most learned scholars, Mr. Grote, are found, where one would scarcely look for them, among the leading bankers of London.

† Sir Robert Peel is assuredly a favourable specimen of an English statesman, so far as being interested in science and anxious to promote it; yet one of his attempts in this line has been so unsuccessful as to compromise him seriously. He was induced to grant an allowance of 300*l.* a-year to Sir James South, in aid of his observatory, a grant which, in twenty-four years, has produced nothing. Though I have heard something of the "party" which misled Sir Robert, I cannot point exactly to the individuals. I believe, indeed, they meant very well; but, as is often the case, were profoundly ignorant of what an observatory is and ought to be. Sir James South was never worth more than 100*l.* a-year as an assistant in an observatory; and though he might have been made useful as a *noter of phenomena*, could scarcely have been otherwise usefully employed. The same money, if assigned to the Observatories of Cambridge or Oxford, would have produced twice as much as it *could* have done in his, unless he had known how to use it properly. In the meantime Smyth, and Dawes, and Lassell, and others whom I could mention, have been passed over; this is unjust, after the grant to Sir James. I own very gladly, that in his later pensions Sir Robert showed discrimination, and that other ministers have been equally judicious; but without the greatest caution, and some good luck, they are always liable to be taken in.

highest scientific rank, neither would they be the safest counsellors.*

While I assert most positively that I am innocent of conspiring against either Sir James South or Mr. Babbage, and that I am certain no such conspiracy ever existed, except in their own imaginations, I do not mean to say that I never spoke, or spoke unfavourably of them. I have already narrated, and without palliation, my behaviour to Sir James and the causes. I will be equally candid towards Mr. Babbage.

I have always expressed my dislike and contempt for his shallow and ill-natured libel, entitled the *Decline of Science*; and with regard to some of his other productions (always keeping clear of analysis), have never scrupled to say that I thought little of them.

I have always spoken strongly in condemnation of his behaviour as Lucasian Professor. I believe the appointment was obtained for him, in his absence, on the understanding that he would discharge the duties. I would not try Professor Babbage's doings by those of Professor Airy and Professor Stokes,—that would be unfair; but what did he do? He examined a few times in eleven years, but I believe he did not print his questions. He said this was very well for Airy, but did not suit him. Now we all know that a good man directs his reading by the questions which have been set in previous examinations, and that in this way, among others, Mr. Airy forced a reasonable amount of physics into the Cambridge studies.

Mr. Babbage never lectured at all, though he once proposed to lecture, and I believe I helped to stop him. He gravely proposed to lecture *immediately after the Senate House examination*, when there is no one in the University; and the bill of fare was to be composed of what he had written (about the Economy of Manufactures, I believe,) for the *Encyclopædia Metropolitana*. This he was to read from the slip. I pointed out to him that it must be a mere Wallis' lecture *at that season*; though I was not candid enough to add, that such lectures would not draw in the University, where no man would go to hear read, and perhaps badly read, what he can read himself in good print in a few weeks. I think, too, the subject did not belong to his Professorship. The Lucasian Professorship is very poorly paid,—not better than the Secretaryship of the Royal Society,—and so hampered by statute, that we can only get a good professor by chance; but I do not see what excuse this is for Mr. Babbage. I do not think he would have been appointed, except in the belief that he meant to perform some duties; for at that time Mr Babbage's general reputation in the University was not high. He had taken no honours,

* Many, many years ago Sir John Herschel made a disparaging remark on *English science*, which has been repeated *usque ad nauseam* since. Possibly the remark was a hasty one, or the reflexion of the peevishness of other persons; it was not acquiesced in, I know, by the most competent persons. This was twenty-five years ago; and if true then, not true now.

had not been a fellow of his college, and, except in the opinion of his personal friends, was in no great esteem. I know there was general dissatisfaction as to his conduct; and that the severity of his requirements from others was thought to contrast very unfavourably with his own performances.

I have often spoken very freely of Mr. Babbage's uselessness and hindrance as a member of a Committee; the reason is, I fancy, that while he is striving to be original, and will talk about what he does not understand, he only succeeds in being odd, or what he himself very properly calls "crotchety."

I have always condemned Mr. Babbage's conduct in not giving a satisfactory account of "the Calculating Machine," and the reasons of its failure, but without pretending to judge of the machine itself, about which I never felt any interest.*

I have always expressed my dislike for Mr. Babbage's quarrelsome and spiteful temper, his readiness to take offence, his implacable hatred, and his inordinate vanity, which completely blinds him as to the consequences of his own conduct. Besides his fatal squabble with the Royal Society, which is the main source of all his calamities, he quarrelled, I believe, with the British Association on some equally trumperry ground, and is, as he admits himself, deemed by his enemies (and let me add by friends too) a "*crotchety, impracticable, disappointed, cantankerous fellow.*" He assumes, indeed, that these epithets are only applied to him because he is more wise, more steadfast, more deep-sighted, and more honest than the rest of mankind.† The French lady who expressed her wonder that she should be the only person who was always in the right, is the closest type I can recollect of such magnificent self-delusion.

I have said already that I cannot recollect any conversation between Mr. Airy and myself touching the *merits* of the Calculating Machine, before the appearance of *The Exposition of 1851*; but I have not unfrequently spoken to him very slightly of

* This is the only real cause of complaint which Mr. Babbage could fairly urge against me. As I had leisure enough, had some mechanical talent, knew the principle of composing and decomposing series by differences very familiarly (few mere amateurs have interpolated more than 1), therefore, it may be said, I ought to have made myself acquainted with Mr. Babbage's inventions, and given them such aid as lay in my power. My answer is, that I did see a rough model of the machine very early, though without any explanation; but I perceived at once that unless I was prepared to bow down and worship Mr. Babbage's idol, I should have a very cold welcome.

† "But I have enemies. It is the curse
Of genius that it cannot spread its wings
And soar triumphant to the welcoming clouds
Without a hateful cawing from the crows.
Mark me! I am not quite as other men;
My aims are higher, more resolved than theirs,
And therefore they detest me. There's no shaft
Within the power of calumny to loose
Which is not bent at me. I am not blind
With soaring near the sun."

So says the "spasmodic" Firmilian: in Mr. Babbage's mouth the speech would be sober earnest.

Mr. Babbage as a member of the Board of Visitors, though I cannot take upon myself to say that the Astronomer Royal agreed with me.

It will be remembered, perhaps, that Mr. Airy exhibited to us a half-sized model of the *Alt-Azimuth instrument*, and particularly called our attention to the support of the base, and its non-liability to *twist*. I don't know whether all remember equally well Mr. Babbage's request to see a *drawing*;* as if that could be, on such a point, a hundredth part as satisfactory as a model; even if the required condition could be expressed by a drawing.

On another occasion Mr. Babbage proposed, that one of the ordinary duties of the Visitors should be to witness an observation made with each of the instruments. He was asked by me, what was to be done when the sky was unfavourable, or when a star of sufficient magnitude to be seen in the daytime did not pass conveniently? and his answer was, that the operation should be performed as if a star was in the field! When I further asked, what advantage he expected from this game of make-belief? I was told that perhaps something would come of it, and that he did not choose to hazard an opinion which might not be justified on trial.† I then remonstrated on the hardship which he wished to impose on better informed persons—to stand by while he was thus acquiring the rudiments of observatory routine,—and suggested that he should visit Greenwich when work was going on, adding, that the Astronomer Royal and his first assistant would feel it their pleasure as well as their duty to give every facility for instruction to any of the Visitors. Mr. Babbage's proposal to play at observing was too ludicrous, and too badly supported, to be entertained.

I will give another instance of Mr. Babbage's *savoir faire*, which occurred at the meeting of 1853. The superintendence of the *Nautical Almanac* had just been given to Mr. Hind, and Mr. Babbage had heard that Mr. Airy had applied for the office. This clearly was no business of ours. If the office *had* been given or *could* have been given to Mr. Airy, and if it had been considered by the Visitors incompatible with his duties as Astronomer Royal, they clearly were called upon to express an opinion, *but not otherwise*. The form of proceeding was in keeping with the subject-matter. Mr. Babbage, who has a curious notion that a *personal attack* upon a member of a committee has a *precedence over all other business*,—over the special business which is set forth in a royal warrant,—assumed a right to introduce this matter *pre-*

* I consider this was mere charlatanism and affectation, and to differ from other people. I have been told that Clement declared Mr. Babbage could not read a drawing; but this must apply to a very early period in their association.

† This is another marked trait in Mr. Babbage. He does not feel that a sincere man of science and a faithful counsellor gives his opinion, if he has one, on the subjects which come before him, without *arrière pensée* or regard to self.

viously to our regular business. This was not allowed, but a good deal of time was wasted in overcoming his pertinacity.*

When the Observatory business was transacted, Mr. Babbage, after requesting that the Astronomer Royal *might not be present*, read a long minute of our proceedings in 1831, already alluded to, in which it was recommended that the superintendence of the *Nautical Almanac* should be withdrawn from Mr. Pond, who was then Astronomer Royal, and who from bad health was not a satisfactory superintendent. I learned afterwards from Stratford himself, that the matter was all arranged by his friends, in order to give him the office; and I should have less objection than I have to *caucusses* and *jobs*, if they were always as well meant and as judiciously executed. The reasoning of the minute was feeble enough; but I suppose it was felt more considerate to rest the case upon the *incompatibility* of the offices, than on the *unfitness* of the existing Astronomer Royal. Be that as it may, Mr. Babbage inflicted the minute upon us (which nobody considered of any weight), and then gravely told us, he had heard that Mr. Airy had applied for the office, and that he wished to know from us whether this report was true. He added, that he had intended to ask this question of Admiral Beaufort, but could not, as the admiral was not present. No one seemed to know, and no one seemed to care, anything about the matter; but I think Mr. Babbage must have learned from the desultory conversation which followed, that the minute of 1831 was held in slight esteem, and that the appointment of Mr. Airy would *not* have been objected to as incompatible with his existing duties. If Mr. Babbage had not *requested* the absence of Mr. Airy, his question would have been immediately answered, and upon that answer he might have proceeded or stayed. The getting the *best*, and indeed *only*, evidence out of the way, the making the *whole* of his scaffolding rest on the *answer* of a gentleman *who was not present*, and this in a matter which was *past* and *gone*, and in *which we had no concern*, shows, that there are classes of men, other than rulers, who justify the sarcasm of the Swedish statesman.†

A very short notice will suffice for the editor of the *Mechanics' Magazine*, the third of the trio.‡

* The Visitors and gentlemen who have been invited to inspect the Royal Observatory generally dine together afterwards, in the English manner. It is essential, therefore, that the business should be finished in good time. In 1853, and, I think, in some former years, Mr. Babbage's incapacity to understand or comply with a regular mode of proceeding has taken up so much time as to derange the dinner hour, and greatly to affect the sociality of the meeting.

† It is a small matter, but I will just hint to Mr. Babbage, who misquotes the saying, and makes the speaker a *Dane*, that there was a great religious war in Germany, 200 years ago, frequently called the Thirty Years' War, that Gustavus Adolphus, king of Sweden, was the great Protestant champion, and his Chancellor Oxenstierna the principal director; and that this *Swede*, Oxenstierna, is the author of the phrase, "*Nescis, mi fili, quantillâ prudentiâ homines regantur.*"

‡ I understand the Editorship of the *Mechanics' Magazine* has passed into other hands. My remarks apply to the person who discredited that office from the discovery of the planet Neptune to the beginning of 1852.

Soon after the discovery of *Neptune*, an anonymous writer, under the signature of "*Exoniensis*," published some remarks on the conduct of the Astronomer Royal in the *Mechanics' Magazine*, charging him with having fraudulently betrayed Mr. Adams's researches to M. Leverrier, moved thereto by his dislike of English science. This stupid libel was forwarded to one or two gentlemen, who, treating it with the contempt it merited, expressed openly their disgust at the article. The author was greatly displeased; and, through some silly acquaintance, attempted to bluster about the matter. My excellent and gallant friend — who was one of the gentlemen concerned, asked me, supposing me to be something of a lawyer, whether he was liable to any *legal* proceedings for the contemptuous language he had applied to "*Exoniensis*." I assured him that an action for libel, at the suit of an *anonymous* person, *himself a libeller*, was an absurdity which no man having a tincture of the profession could dream of. As I was just putting the last hand to a small publication, in reply to a letter by Mr. Babbage on the subject of *Neptune*, I appended a note, saying, that the letter of "*Exoniensis*" was so ridiculous that I supposed it was a hoax.

So far as I know, neither the Editor nor "*Exoniensis*" have retracted this foul calumny against Mr. Airy, or expressed any contrition, though it is impossible that they should not know how utterly false and absurd that calumny is.* I fancy the Editor felt nettled by my remark, and, with the meanness of persons of his kidney, has lent himself to the calumnies of MM. Babbage and South, though he might have seen that they were probably untrue.

I have already pointed out a discovery of the Editor, which has escaped our notice as Visitors for nearly twenty years, that the present Astronomer Royal has neglected the duties of his office, falling greatly short of his predecessors, and that Mr. Main's services are merely those of chaplain to the establishment. *Ex pede Herculem*; the intellectual stature of the Editor of the

* I need not remind the members of the Board of Visitors of what they know so well, but I may inform the uninitiated, that Mr. Adams's case rested *solely* on the *evidence* of the Astronomer Royal, *voluntarily* and *spontaneously* produced. The whole difficulty arose from Mr. Adams's neglect in not replying to the letter in which Mr. Airy asked, whether the new planet would account for the irregularities in the radius vector as well as for those in the longitude of Uranus. Mr. Adams could have answered this query in five minutes, if he could have overcome the dislike he has to writing at all. To this remarkable idiosyncrasy all Mr. Adams's correspondents can testify. A respectable critic, who did not, however, understand the bearing of Mr. Airy's question, nor the facility with which it could have been answered, assumed, rather ignorantly, that it was proposed in a carping spirit. Fortunately, the more intelligent portion of scientific Englishmen, while they admitted M. Leverrier's merits with candour and admiration, advocated those of Mr. Adams with patience and temper, and thus secured a just appreciation of his claims, which his foolish partisans (British lions) would have swamped in a personal or national dispute, and thought themselves very clever and spirited.

Mechanics' Magazine may be correctly meted by these fragments, and his veracity and modesty are doubtless to match.

Of the three parties, each one thinks he has received some injury from me and has joined with the other two to revenge it; whether by an actual conspiracy, or prompted by a community of feeling and disposition, I cannot say. Mr. Babbage has dwelt upon his defeat in the matter of the great equatoreal, until it has affected his mind and deprived him of all judgment where I am a party concerned. We know on the highest medical authority, that "a long indulgence in dreams of vanity and pride may upset a vigorous intellect," and "that a man may allow his imagination to dwell on one idea, until it acquires an unhealthy ascendancy over his mind." I can only account for the hallucinations contained in the twelfth chapter of *The Exposition of 1851*, on the supposition that Mr. Babbage is a monomaniac at least, though, like other monomaniacs, with some method in his flightiness.

I have heard, and believe, that he has made considerable efforts to stir up Sir James South's anger against me, — no very difficult matter I dare say, and I suspect that the embellishments of "the Knight's Tale" have been hatched under the fostering incubation of Mr. Babbage; I don't mean that the falsehoods are of his *suggestion*, — far from it, but that Sir James's imagination has become more vivid and his memory more retentive under Mr. Babbage's encouragement. When the story had arrived at a proper degree of maturity, I fancy that the cat's-paw was urged to publish it, and that the *Mechanics' Magazine* was a willing and suitable vehicle.

It may be asked, why I have not rather appealed to a court of law than to public opinion; and certainly I might have proceeded against one, if not all, of the parties with success and amusement. But I should be sorry to inflict a pecuniary penalty on either Mr. Babbage or Sir James South: nor could I well do so, as I have received no *damage* beyond the expense of this publication. MM. South and Babbage are chartered libellers; they have followed the trade for the last quarter of a century, and might almost plead a prescriptive right to continue it. But besides this, even if the mental disease of the one, and the imbecility of the other, were less decided, I feel myself quite competent to defend myself, leaving them the choice of weapons. As to the Editor, the case is somewhat different; but one may be libelled a good deal by such people before one knows or feels it. In compassion to his readers, perhaps — but that consideration does not press; and readers who can be so easily bamboozled scarcely deserve pity.

I have been so busy and so much indisposed, and so thoroughly disgusted with my task, that I have only discharged it at last as a matter of necessity. I am thoroughly ashamed of my consorts, and am almost fearful of being classed in the same category with them, because, in self-defence, I have used fairly and *truly* the homely language in which I have been assailed. That men of our age (I am the youngest of the three principals, and on the shady

side of threescore) should be thus engaged, presents anything but an agreeable picture ; and I wish I were out of the group. I can only say that I have been dragged into this shindy against my will ; and if my adversaries find themselves with broken heads, it is not my fault.

“ Ma chi urta col muro è suo 'l dolore,
E la materia torna sopra 'l matto.”

I do not believe that any man who can reason on evidence and probabilities will entertain Sir James South's calumnies for an instant.* My *direct* evidence would, I trust, outweigh his *hearsay*, even if the indirect and circumstantial proofs of his falsehood were wanting. I have, besides, great confidence in the effect of a *true* story over a *false* one, when the hearer is intelligent. I think, too, that most of the statements in this pamphlet have been, at one time or another, made to different members of the Board ; and they can therefore judge of the *probable* truth of my story, from its being *substantially* what it always has been. I have not, to my knowledge, mis-stated or over-stated anything ; and though I dare not assert that I have made no mistakes, I am sure that what I have said is very nearly the literal truth.

But though I have small fear of my veracity being disputed, and still less that I should be suspected of meanness in money matters, I shall, I fear, hardly escape one imputation, which, if true, would be a grievous one to me, — that I am ill-natured and revengeful, and that I have snubbed Sir James and “shown up” Mr. Babbage from pique : yet I have not been the assailant, and it has been with great reluctance that I have screwed myself up to the sticking-point of publication. If I have treated Sir James rather roughly, it is not so roughly as he deserved, nor so roughly as I shall treat him, if I see occasion to chastise him a second time. It will then be known *how much* I have spared him now ; *how long* I have done so is evident from the lateness of this exposition, which, if I had been ordinarily spiteful, would have appeared twenty years ago.

There were other persons, besides Mr. Babbage, who gave evidence as blundering as his, and who have no claim on my consideration ; but I have avoided “showing them up,” because it is not necessary to my own vindication.†

I deny malice to Mr. Babbage. I dissuaded him from lecturing when he *could* have no audience, — a ridiculous idea which an ill-natured man would have encouraged. After our quarrel I did

* Great allowances must be made. A man of coarse perception, indifferent education, and violent temper, hardly *can* speak truth, if he wishes it. Perhaps, too, Davies Gilbert's suggestion, that Sir James is a little cracked, may be true. Certainly his recollection does not serve him very correctly, or he would scarcely have ventured to apply to himself the phrase, “My ways are ways of peace,” which he did with considerable unction (and to the amusement of his auditors) when he was giving evidence on the arbitration.

† If Sir James had ventured to publish his promised account of the arbitration, I should, probably, have been less lenient.

not publish his evidence on the arbitration, yet what would have been more likely to be injurious to him as a mechanical projector than the proof of his mechanical blundering? If I have shown him up at last, it is only when it was necessary to my own defence, and when, by espousing the calumnies of Sir James South, he had made them his own. If gentlemen could put themselves into my place, I am sure they would wonder at my *forbearance*.*

I believe, however, that I have not been actuated solely, or even principally, by personal motives. I abated Sir James South because he was an acknowledged *public* nuisance; he was no nuisance to me. I made him pay his debts; but no halfpenny came into my pocket. It is not easy for a man to be sure of his own motives, but I *believe* that mine were rather kindly feelings and respect to others, and a love of justice, than spite towards Sir James. The proof is, that when he was no longer troublesome he was no longer troubled. When he took to the appropriate amusement of compounding *fireworks*, I gave him no annoyance.

I protest, therefore, most strongly, against Mr. Babbage's hypothesis, that hatred of Sir James South, or of himself as the friend of Sir James South, has been the mainspring of my conduct for the last five-and-twenty years. It is from a defect in his idiosyncrasy, which leads him to judge of other people by himself, that this idle and mischievous delusion has arisen. Perhaps when he looks about for my "party," and cannot name one living man who will not contradict him, he may begin to suspect that he has not "escaped the hopeless obscurity in which he was originally involved." If Mr. Babbage could lay aside his morbid vanity and intense malignity he would see things as they are, and as, I believe, I have represented them.

I fear that there is no hope that Mr. Babbage can be undeceived by any amount of evidence, nor does it personally concern me. But if any of his friends think him still pervious to reason, I am quite willing to justify myself against his complaint of persecution. There are at least half a dozen gentlemen of the highest character, who know us both in various degrees of intimacy. To one or any of these I will answer any charges he may make, which are *definite* enough to admit of answer. I have already challenged him to specify a single member of the "party," which, in his belief, has been in league with me. Mr. Airy, who is the only person named by Mr. Babbage, cannot recollect, any more than I can, that we ever held a conversation upon the subject of the Differential or Analytical Machines before the publication of *The Exposition of 1851*.

The slender peg on which Mr. Babbage hangs his erroneous convictions is this, that in 1831 I threatened to put him down as a partisan of Sir James South, with whom I was at that time in feud. I regret that I used, in my heat, an expression which has

* The charity which every man shows in forgiving his neighbour's wrongs, and the liberality with which he dispenses his neighbour's fortune, would be admirable, *if the feeling cost anything*.

affected Mr. Babbage in a way I never intended.* As I was angry at the time, I probably meant little or nothing seriously; but so far as I can recollect, it was this. At that time very few people indeed, probably not half a dozen in all England, could be said to be tolerably acquainted with the theory and practice of astronomical observation, and Mr. Babbage certainly was not one of the number. If I had any very definite meaning, I fancy I must have alluded to this ignorance in Sir James South and his supporters, and to my power, and perhaps to my intention, of exposing it. Possibly I might have some vague feeling as to the Lucasian Professorship. But as Mr. Babbage and I continued on the same footing for four years after this occurrence, as we had been before, and as acts of courtesy, I believe, passed between us as before, I cannot allow that a worse interpretation ought to be put on my words than I have suggested. Our quarrel must be dated from the summer of 1835, and from that time to the present year 1854, I am not conscious of having given Mr. Babbage the slightest reason for just complaint.

As the Royal Society declined to hear what I had got to say,† I have no intention of forcing myself upon its notice, though I shall be quite ready to defend myself, if any fellows, *after reading what I have here written*, shall require further explanation, and can ensure me a patient hearing. I can have no doubt of the result which every unbiassed and clearheaded gentleman must come to. After what has passed, I shall confine myself to self-defence, if I am assailed. I do not want the incumbrance of any help or protection, but merely a fair hearing.

As Mr. Babbage has twice agitated the Board of Visitors, though in vain, it is, I think, clear that he and I *ought* not to be components of any Board which requires the honest co-operation of its members. I have, I conceive, cleared myself of the imputations which can be considered disgraceful, but that I refer to the Board of Visitors. If three members (MM. Babbage and South

* I should be infinitely sorry if I thought Mr. Babbage's delusions were occasioned by this idle speech, but I am sure that it has merely come in aid of a "foregone conclusion."

† I am quite aware this was an act of timidity, a feeling to which large meetings are very subject. People do not like to be forced to listen to a story and to decide. There are, too, a few members who slipped into the Royal Society in olden times, without any motive (certainly without any scientific motive) that one can see, and who are always on the watch for an opportunity to display their imagined talents. By the mode of election introduced a few years ago, such intruders will in future be kept out. The advantage of this change is evident. If the Council had asked the consent of the Society for the new arrangement, and if the balloting lists contained all the names thought *eligible* by the Council, and not a *specified number*, I, for one, should have been rather better pleased.

Perhaps a small standing Committee, selected from our choicest fellows, would be more competent to prepare the balloting lists than the fleeting Council, and the appointment to such a Committee would be the highest honour we could give. As the Royal Society is not amenable to the courts of law for its management, but to the Lord Chancellor as *visitor*, the question whether the limitation in numbers is in accordance with the Charter, is one of mere curiosity. I have never understood the advantage of the rule.

excepted) will signify to the President their wish that I should retire, I will do so, without asking for reasons, or names, or disputing their motives. At our next meeting I shall ask if such intimations have been received, and how many, that I may act accordingly.

APPENDIX. No. I.

Extracts from the Statutes of the Royal Society. 1840. 8vo.
London.

CHAPTER II.

In the obligation subscribed by each member, each Fellow declares "that he will observe the Statutes and Orders of the said Society."

CHAPTER V.

Of the Causes and Form of Ejection.

"I. If any Fellow of the Society shall contemptuously or contumaciously disobey the Statutes or Orders of the Society or Council, or shall, by speaking, writing, or printing, publicly defame the Society; or advisedly, maliciously, or dishonestly, do anything to the damage, detriment, or dishonour thereof, he shall be ejected out of the Society.

"II. Whensoever there shall appear to be cause for the Ejection of any Fellow out of the Society, the subject shall be laid before the Council; and if a majority of the Council shall, after due deliberation, determine by ballot to propose to the Society the Ejection of the said Fellow, the President shall in that case, at some ordinary meeting of the Society, announce from the Chair such determination of the Council; and at the meeting next after that at which the said announcement has been made, the Society shall proceed by ballot to determine the question; and on its appearing that two-thirds of the members present have voted for the Ejection of the said Fellow, the President shall proceed to cancel his name in the Register," &c.

In an earlier edition of the Statutes the form is more simple. The President proposed the Ejection at any meeting, and the Society proceeded immediately to ballot. Notice seems not to have been necessary.

CHAPTER VI.

Of the Election of the Council and Officers.

The business of Anniversary Meetings seems to be the election of the Council and Officers, and nothing else; and any discussion relating to other subjects is probably irregular. It is, however, so convenient a time for questions and explanations, that in ordinary cases one would be sorry to see the President exercise his office too strictly (he is "to check irregularities and to keep all persons in order"). At the ordinary meetings, no discussion is allowed of any subject not immediately connected with the usual business, viz. "to read and hear letters, reports and other papers, concerning philosophical matters."

CHAPTER XII.

Of Special General Meetings of the Society.

" I. The President or Council may at any time call a Special General Meeting of the Society when it may appear to them to be necessary.

" II. Any six Fellows may, by notice in writing, signed by them, and delivered to one of the Secretaries at an Ordinary Meeting of the Society, require a Special General Meeting of the Society to be convened for the purpose of considering and determining on the matters specified in such requisition.

" III. The Council shall, within one week after such requisition shall have been so delivered, appoint a day for a Special General Meeting accordingly, and give one week's notice thereof to each Fellow residing within the limits of the threepenny post, stating in the notice the object of such meeting. At such meeting no business shall be brought forward except what shall have been so notified."

It is quite clear, from the above extracts, that if Mr. Babbage thought I ought to be ejected from the Royal Society, he could only apply to the Council. But when his accusations met with no attention, he had no right to break the Statutes and Orders of the Society; that he was permitted to do so shows some want of presence of mind in the President. There was, besides, no pretext for asking any question of me through the President for the purposes of explanation. I had replied to the very same question from the same person, under the same President, at the Greenwich Visitation a few months before; and, I believe, quite to the satisfaction of every one present, except Mr. Babbage.

When a particular course is marked out by law, it is questionable whether any other course can be followed. *Perhaps* Mr. Babbage might have proceeded under the second paragraph of Chapter XII., and called for a Special General Meeting; a course to which I should have raised no objection, but contented myself with requesting that the more *recent* cases of MM. Babbage and South, " for publicly defaming the Society, by speaking, writing, and printing," should be previously or simultaneously entered into.

Mr. Babbage might plead, with some plausibility, *autrefois acquit*, if the following extract from page 19 of Sir James's *Thirty-nine Charges* is to be relied on:—

"The following note is from *The Times* of July 8 (1830): 'At a meeting of the Council of the Royal Society, held in the Society's apartments, on the 10th of June last, the Secretary has recorded the following speeches: 'Captain Kater stated, that he wished to take the liberty of proposing a question upon a subject in which he conceived the dignity and well-being of the Society were concerned. As the Charter invests the Council with the sole government of the Royal Society, and the exclusive management of all its concerns, he conceived that one of the first duties of the Council was that of preserving the statutes inviolate, and of noticing any infringement of them. He therefore requested to be informed whether any and what steps were intended to be taken respecting a publication by Mr. Babbage, entitled, *On the Decline of Science in England*. The President thereupon observed, that deeply as he regretted the injurious tendency of Mr. Babbage's publication, and disapproved of the uncandid spirit which pervaded it, and notwithstanding the violation of the statutes, which had in strictness subjected its author to the penalty of ejection from the Society; he was yet unwilling, in consideration of the past services which Mr. Babbage has rendered to science, to proceed to this extremity; but thought it would be more consistent with the dignity of the Society to wave all further notice of this matter. Captain Kater replied, that no one could have a higher respect for Mr. Babbage's scientific attainments than he had, and that he had been in habits of the most friendly intercourse with him for many years. Nothing but a sense of duty had prompted his bringing this matter before the Council; he felt it, however, also to be his duty to *bow with implicit deference* to the opinion of the President, and would therefore refrain from offering any further remark upon the subject.' Present on this memorable occasion, 'Davies Gilbert, Esq., President,

in the chair; Mr. Charles Bell, Mr. Robert Brown, Mr. Frederick Daniell, Mr. Henry Ellis, Captain Kater, Captain Sir John Franklin, Dr. Philip, Mr. Pond, Mr. George Rennie, Dr. Roget, and Mr. Warburton.' Sir James then adds, *from himself*,—"Really, on finding that some of the above persons joined in such a proceeding, one is led to suspect that the atmosphere of the Royal Society Council-room must contain some pestiferous principle." If Sir James were an authority, this remark would imply an agreement of the Council with the President, and consequently a sort of pardon.

But Sir James could not escape, if there is no *statute of limitations*. His *Thirty-nine Charges* were only ignominiously cast aside, and no one thought him worth ejection. As to myself, I was not a fellow for many years *after* the time of the charge against me, so that the case, if it be one, is not provided for.

Seriously speaking, however, for the Royal Society is not strong in mooted points (and I trust it never may be), if I had been allowed a hearing, I should have proposed that the Council should appoint a small committee to decide, first on the truth or probability of Sir James South's accusation, and then to point out who of the three persons concerned should be requested to quit the Society. There is no law or custom, that I ever heard of, which would apply to my case (even if Sir James South's figments were believed, which I cannot think possible), but I should be sorry to continue a member of any society in which even a moderate minority were of opinion that I was *de trop*. The gallant knight, indeed, said of the Royal Society, that "where **ADMISSION IS NO HONOUR, EXPULSION CAN BE NO DISGRACE.**" I boast no such indifference, and deny his aphorism, according to which, being kicked out of a public room or omnibus would be no injury. I should quit the Society with regret, but should not feel that the *disgrace* attached wholly to me.

Is it not rather odd, that the persons so jealous for the honour of the Royal Society should themselves have barely escaped expulsion for a manifest breach of statute; one in consideration of his past services, and the other because he was not deserving so much attention?

APPENDIX. No. II.

Special Meeting of Council, March 3, 1831, for considering the Bye-laws, &c. Mr. F. Baily in the chair.

(Sir James South *not present*.)

The Bye-laws were taken into consideration.

Letters from Mr. Plaskett and Mr. Hoppe were read, "from which it appeared probable that the Charter of the Society would be signed, and the Society commence its existence as a Corporate Body, before Friday the 11th of March, 1831, the next ordinary day of meeting.

"That a Special General Meeting be summoned for March 11, 1831, to meet after the business of the ordinary meeting of that day shall be concluded, to make and establish such Bye-laws, and to elect Officers for the ensuing year, according to the provisions of the Charter.

"That the Secretary shall issue circulars convening such Special General Meeting."

The Charter received the sign manual on March 7. 1st of William IV. The circulars were detained for twenty-four hours by Lieutenant Stratford, in order that he might see Sir James South, and give him the first information. I am pretty certain that Sir James's absence from the Council was a *calculated* and *premeditated* proceeding, in order that they might feel in a difficulty, and be induced to *propitiate* him some way or other.

Meeting of Council, March 11, 1831.

Mr. F. Baily in the chair.

Dr. Lee.

Mr. De Morgan.

Mr. Wrottesley.

Capt. Smyth.

Capt. Beaufort.

Mr. Riddle.

Sir James South.

Mr. Stratford.

"Resolved,—That the Chairman be requested to state to the Ordinary Meeting of this evening, at the close of the business, that the Charter has been received and now lies on the table.

Also, "That the Council having circulated the notice, which, it is presumed, has been received by all the members present, we are now a General Meeting, to decide whether the Charter shall be read and accepted; but previously to determine on the proceedings which shall be taken."

Also, "That the Council have passed the following resolution:—

"That it be recommended to the present General Meeting to proceed to elect the Council for the current year.'"

It was on the evening of the same day that Sir James disturbed the meeting, as narrated in the text, and violated the arrangement of the Council, to which he had been a consenting party three or four hours before.

Special General Meeting of the Society, March 11, 1831.

"The business of the ordinary monthly meeting being concluded, the Chairman (Mr. F. Baily) stated, "That the Society must consider itself as the Special General Meeting summoned by the circular to meet that evening.

"After some discussion relative to the business for which the meeting was convened, some doubts were expressed whether the meeting had been properly summoned; when it was resolved that this meeting do now adjourn."

Meeting of the Council, March 19, 1831.

(Sir James South *present*.)

"A form of circular convening a Special General Meeting for the 6th April next ensuing was laid on the table, and having been read and approved of, was ordered to be printed and to be issued on the 28th March next. The following is a copy of the circular:—

"Sir,—I have the honour to inform you, that in consequence of some doubts having arisen as to an informality in summoning the Special General Meeting for the 11th instant, no business was transacted; and that another Special General Meeting of the Society will be held on Wednesday, April 6, at three o'clock in the afternoon, for the purpose of deciding on the acceptance of the Charter, and in case of such acceptance, for electing the Council for the current year, for determining on the Bye-laws by which the Society shall then be governed.'"

Meeting of the Council, April 6, 1831.

"Mr. Riddle laid before the Council a letter from Mr. Barlow, intimating that, from his other engagements, he cannot attend the meetings of the Council; and Mr. Stratford having stated that Sir James South was desirous of retiring for the present year, it was resolved to recommend to the General Meeting of this day to substitute in the balloting lists the names of Dr. Tiarks and the Rev. R. Sheepshanks for those of Mr. Barlow and Sir John South."

APPENDIX. No. III.

I suspect that many persons who profess to have a great admiration for the *Calculating Machine* really know very little of its functions, and I am quite sure that the public in general is wholly ignorant of the matter. I will try to give some idea of the results which might be expected from it, though, as I never examined the machine, or read any description of it, I can say nothing as to its contrivances or mode of operation.

Take any series—such, for instance, as the squares of the natural numbers, 1, 2, 3, &c.,—and arrange as follows:—

The squares are	1	4	9	16	25
Subtract each from the following, the <i>first</i> } differences are.....	3	5	7	9	
Repeat the operation, the <i>second</i> differences are	2	2	2		

As the second differences are equal, the *third* differences are 0.

Now the problem to be solved is to *recompose* the *original series*, having only the first figure in each horizontal line; i. e. the first term 1, the first difference 3, and the second difference 2. This requires merely a reversion of the previous process of *decomposition*.

Write down the constant second difference, which is here 2, as many times as you please.

To reproduce the first order of differences, set down 3, which is known or given; also write 3 under the first 2, and add, which give 5; write 5 under the next 2, and add, which give 7, and so on: these are the *first* order of differences.

To reproduce the original series, set down 1; also write 1 under 3, the first number of the first order of differences, and add, when you have 4; write 4 also under 5, which is the next member of the first order, and add, you have 9; write 9 under 7 and add, when you have 16; and thus the original series is recomposed out of 1, 3, and 2; the general direction being, that you add the number last obtained to the following number in the differences.

Second order of differences	..	2	2	2	2
		3	5	7	9
First order of differences	3	5	7	9 11
		1	4	9	16 25
Series required	1	4	9	16 25 36

Now the *Calculating Machine*, as I understand it, will just perform the same thing, having the same data. Supply it with the first term 1, the first difference 3, and the second difference 2, and it would, if it were finished and capable of work, perform some equivalent operation to that which I have just described.

If the first number be 1, the first difference 7, the second difference 12, and the third difference 6, an exactly similar process will reproduce the cubes of the natural numbers, viz. 1, 8, 27, 64, &c. The number of orders of differences does not alter the process, however numerous they may be.

So far as I know, the *Calculating Machine*, if completed, would not perform any higher or more complicated operations than such as I have described, though its use would not be limited to such simple series. I believe, indeed, that its powers of addition are confined to *decimals*, and if so, there are many tables which would not fall within its scope.

I have no doubt at all that the *Calculating Machine* contained many ingenious contrivances, and was a specimen of most beautiful workmanship. It occupied Mr. Babbage, and some of the very best workmen in England, for many years, and cost about 17,000*l*. It is, perhaps, to be regretted that circumstances, not yet fully explained, put a stop to the machine before it was completed, and when the principal cost must have been incurred. Still I cannot help feeling sceptical as to the *practical* utility of the machine, even if it had been completed. The series to which it would have been most applicable have long been calculated, and the computations it would have furnished are of so elementary a nature, that they could be supplied, if wanted, at a very cheap rate indeed, and by persons of the very lowest capacity. It would save no *intellectual* labour.

Of Mr. Babbage's second machine, which he calls the *Analytical Machine*, I can say nothing; it is not as yet *in rerum naturâ*, nor has any intelligible account of it been published by the inventor himself, in this country—at least to my knowledge. That Mr. Babbage has the highest admiration for this child of his own brain, is certain; and some other persons, relying on his representations, seem to believe in it too, though I cannot make out that any one understands it. What we are told, except some very vague notices in the *Ninth Bridgewater Treatise*, and in *The Exposition* of 1851, is by a *foreign* professor translated by an English Countess. Would any rational man introduce his discovery in this irregular and indirect manner? When I remember the extravagant expectations held out with regard to the *Calculating Machine*, I am not quite satisfied that the *Analytical Machine*, if it could be executed, would justify what has been promised for it; neither is it certain it could be executed, were Mr. Babbage's life sufficiently prolonged, and if he did not meanwhile invent some third machine to which the *Analytical* would have to yield in its turn.

Notwithstanding "the great confidence the Whigs had in my devotion," my opinion was never asked about Mr. Babbage or his machines. If it had been, I should have referred my querist to Mr. Airy, or Professor Stokes, Professor Willis, or Professor Miller, or some *firm* man who understood the subject; I should not have given any opinion myself, for the simple reason that I know nothing about the matter, and am too indolent and too indifferent to acquaint myself with it. If my friends had treated me Proteus fashion, and applied thumb-screws, I should have advised that the *Calculating Machine* should be finished under Mr. Babbage's direction (with, perhaps, some little control over the purse), and that the second, or *Analytical Machine*, should be postponed till the completion of the former. Although the machine might have been of little value for actual calculation, it would have been something to have had a piece of machinery which would calculate. The country would have been better satisfied, I think, and we should at least have had a clever toy for our money. It is very likely that some of the contrivances would have found useful applications elsewhere, and that the novelties would have fructified in other brains. The possibility of these indirect advantages should always reckon for something when new schemes are propounded. I have understood that, in the hands of Mr. Clement, the construction of the *Calculating Machine* formed a school of better workmanship than had hitherto existed. If it tended to develope the talents of Mr. Joseph Whitworth, the cost has been amply repaid.

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CORRESPONDENCE

RESPECTING

THE LIVERPOOL OBSERVATORY,

BETWEEN

MR. JOHN TAYLOR

AND

THE REV. R. SHEEPSHANKS, M.A.

FELLOW OF TRINITY COLLEGE, CAMBRIDGE;

MEMBER OF THE BOARD OF VISITORS OF THE ROYAL OBSERVATORY;

FOREIGN SECRETARY OF THE ROYAL ASTRONOMICAL SOCIETY;

F.R.S., F.G.S., F.C.P.S., ETC.;

AUTHOR OF THE ARTICLES ON PRACTICAL ASTRONOMY IN THE PENNY CYCLOPEDIA.

"I did never know so full a voice issue from so empty a heart; but the saying is true,—
The empty vessel makes the greatest sound.

"You must learn to know such slanders of the age, or else you may be marvellously mis-
took."—*Shakespeare*.

LONDON:

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LEICESTER SQUARE.

1845.

and a cause of great annual expense, besides occasioning much responsibility for its management and direction. It could not have been more efficient for the improvement of navigation than that which you have erected; and, most of all, it would not have been an *example* which less wealthy bodies may follow. When the advantage which Liverpool will certainly derive from a perfect and perpetual knowledge of Greenwich time, and from a superior choice of chronometers, is widely known, and also the moderate cost at which this may be secured, we may reasonably expect that small, but competent, observatories will be established at every considerable port from which deep-sea voyages are undertaken.

I am, Gentlemen,

With very great respect,

Your obedient Servant,

R. SHEEPSHANKS.

PREFACE.

THE analysis of Mr. Taylor's criticisms on the Liverpool Observatory, and the examination of his pretensions, literary and scientific, have filled so much space that I will be as brief here as possible.

On receiving the anonymous attack upon my portion of the *Report of the Council of the Royal Astronomical Society*, I immediately forwarded a defence to the editor of the *Liverpool Mercury*. The friend who executed my commission was told that as the press of matter that week was very heavy, the insertion must be postponed till the week following. But when the paper next week contained neither my letter nor any notice to correspondents, I wrote, as follows, to the editor:—

“ To the Editor of the Liverpool Mercury.

*“ 14 London Road, Reading,
April 19, 1845.*

“SIR,—In your paper of the 4th instant, a letter, signed T. and dated the 1st April, impugned certain statements (relative to the Liverpool Observatory) contained in the annual report of the Royal Astronomical Society of London. As the author of that portion of the report, I forwarded to the office of your journal, through the hands of Mr. Hartnup, the director of the Observatory, a reply to T.'s strictures; this reply was not inserted on the 11th, owing, as I was informed, to a press of matter, and on the 18th it is again omitted.

“Having thus, to save trouble, briefly stated the facts, I beg to know on what ground this omission is made.

“ Your obedient servant,

“ RICHARD SHEEPSHANKS.”

This note was not acknowledged.

A copy of the reply was taken to the editor of the *Liverpool Times*, who very kindly inserted it. See page 19.

Not being conversant with the habits of gentlemen of the press, I was perhaps unnecessarily surprised to find that the editor of the *Liverpool Mercury* had not only deferred *sine die* the publication of my defence against an attack made in his own paper, but had handed over the MS. to Mr. Taylor. If this mode of proceeding be usual and considered fair in the craft, I have no peculiar right to complain; but it seems to me that some public notice should be given of the practice, which is unquestionably dishonest. Mr. Taylor, indeed, saw nothing to be ashamed of, for, with a remarkable degree of candour, he states the fact openly in his letter (p. 22), and it would appear without any perception of the discredit which certainly attaches to his share of the matter.

The correspondence which ensued (when the kindness of Mr. Baines, the editor of the *Liverpool Times*, had opened me a way to be heard) is here published exactly as it occurred, until Mr. Taylor, finding himself defeated at all points, quitted the field. After this, the following notice to correspondents appeared in the *Liverpool Mercury*, but *à propos* to what, I cannot say:—

“THE REV. R. SHEEPHANKS.—This personification of the *cacoe-thes scribendi* may be assured, that had we been able to find sufficient space for his first moderate letter, it would have appeared at once; but the fact is, that far more important communications than his, which were in type before it was even written, have not yet gained a niche in our columns.* We now rejoice at our escape from any connexion with his subsequent productions, for of all the specimens of arrogant assumption and Billingsgate language we ever met with, his writings present the fiercest concentration. They are utterly unbecoming a gentleman or a clergyman, and prove the author to be even less acquainted with the virtues of good temper and humility, which ought to adorn a Christian minister, than he confesses himself to be with the mathematics.”

I have no hesitation in attributing this paragraph to the

* I do not believe the editor's plea of want of sufficient space, as he could always find room for Mr. Taylor's communications, which he will scarcely say were “far more important than mine;” and if I *did* believe him, the de-

pen of Mr. Taylor. The pointless allusion to my profession, the vulgar reproach that I am no gentleman,* and the misquotation and misrepresentation, which use has made his second nature, plainly designate the author, who manifests, moreover, a degree of soreness very natural to Mr. Taylor, but quite uneditorial. Though shot under the shelter of Ajax Smith (or whatever his name may be), the shaft is surely from the bow of Teucer Taylor. A sevenfold shield is not needed against such an assailant.

To the editor of the *Liverpool Mercury* I have nothing further to say, "*Cum suis vivat valeatque doctis*," and he may perhaps learn a little fairness, or, at least, caution, from this exposure; but I will remind Mr. Taylor that in this short paragraph, besides the useless falsehood already noted, he has made the *inconsistent* charges against me of arrogant assumption and voluntary confession of ignorance. The *assumption* consists in my taking for granted, after I had proved it, that my knowledge of astronomy, if not half as extensive,† is rather more exact

fence is not valid. Having admitted Mr. Taylor's attack, an honest man could have no choice about publishing the defence, however inconvenient it might be. As my language is allowed to have been unobjectionable, he has no subterfuge left, except he asserts the right of "gentlemen of the press" to calumniate with impunity. The truth I believe to be, that he and Mr. Taylor put me off in the hope that delay would disgust me; at any rate, by sending me to another newspaper, they secured, in a great measure, a one-sided hearing, a grievance which I can only imperfectly remedy by this publication. I acquit the editor of the rest of the notice, but an English tradesman ought not to have countenanced the piratical trick of firing under false colours.

* "I s'pose," said Sam, "I s'pose the other gen'l'men as sleeps here *are* gen'l'men?"

"Nothing but it," said Mr. Roker. "One of 'em takes his twelve pints of ale a-day, and never leaves off smoking, even at his meals."

I should make a very sorry gentleman on Mr. Roker's model, but the twelve pints and continuous smoking would scarcely be more nauseous than "performing Kootoo" to the Liverpool Potts, or believing in the scientific infallibility of Mr. John Taylor, which are included, I presume, in *their* definition of "gen'l'men as *are* gen'l'men."

† Mr. Taylor, with a license beyond the poetical, has boasted of his extensive reading (see p. 23), but it has not appeared when "there was need of such vanities." Granting the truth of his claim, he may fairly brag that he has forgotten at least *twice* as much as I know.

persons may be found (silly enough, to be sure, but not otherwise contemptible), who think him something of an authority. Now, if these points be all granted, it follows that Mr. Taylor, with every disposition, has some power, to be mischievous. Therefore he is a nuisance, and ought to be abated; and as no other person would undertake the task, I have.

I beg next to remark that Mr. Taylor was the assailant, and was the first to use uncivil language; and while I agree with Fluellen,* that I could not justify myself in following his example throughout, yet it does warrant my giving a very *frank* opinion about him. I have not, to my knowledge, misstated or misquoted any authority, pushed any argument, or exaggerated a tittle; and if, after reading Mr. Taylor's letters to Lieut. Stratford and to Messrs. Dawes and Lassell, any one holds that I ought to have used more honeyed words, he has clearly the advantage of me in forgiving the insults offered to his neighbour. If I could have attained my purpose of taking the sting out of Mr. Taylor without giving him pain, I would have gladly done so; but it is not in the nature of things, any more than to apply the cat-o'-nine-tails to the sufferer's liking, or to make rhubarb and senna pleasant drinks.

I shall be reproached by my friends for wasting my time on so insignificant an object; but I do not allow that a man's intellect is any fair measure of his capacity for doing harm. If Mr. Taylor can pass for a man of science, even in the second class of a provincial town, it is not easy to estimate the possible mischief he may cause; and I know no way to prevent this, except by shewing that he is *ridiculously* ignorant. To astronomers I make no apology; at their hands I only expect thanks, and most particularly from Father De Vico.

I here close with pleasure my personal altercation with Mr. Taylor, and hope that the lesson he has received may lead to his amendment, or, at least, secure the peace of others.

The Liverpool Observatory, though not faultless, is perfectly

* "If the enemy is an ass, and a fool, and a prating coxcomb, is it meet, think you, that we should also, look you, be an ass, and a fool, and a prating coxcomb: in your own conscience now?"

sufficient for its designation; the scale and equipment of it are strictly in accordance with the opinion of the Astronomer-Royal, and the instruments are made under his personal superintendence. The truth is, that new *standard* observatories are not needed in Great Britain; those now in activity—Greenwich, Cambridge, Oxford, and Edinburgh—are quite sufficient. The real want is more assistants to work the instruments completely. The especial object of the Liverpool Observatory is to improve navigation: as this purpose required a nice and conscientious observer, the equatorial telescope was added to find him full and useful occupation. To find fault with an establishment because it is not something quite different, is foolish and unfair. Chronometers are as well rated and time is as scrupulously determined at Liverpool as at Greenwich; this is the main object, and one of vast and increasing usefulness. Something more has been done, and more, doubtless, will be done; but I will not invade Mr. Taylor's province of prophecy.

I deem Mr. Taylor's account of his *noble* observatory and his progress therein to be apocryphal. If he proposed to the Lords of the Admiralty that the port of Liverpool should build and endow an observatory, giving the patronage and direction to their lordships, he would get, no doubt, a civil answer for the compliment. But this is all, so far as I can understand him, which he professes to have done, and it is a small step. The connexion between an observatory such as Mr. Taylor saw in "vision beatific" and schools for navigation, &c., is very remote. The instruments and methods of a navigator and an astronomer are very different, and a seaman would learn almost as little from an observatory as an observer would learn by going to sea. So much of his plan as refers to the education and qualification of seamen is not at all dependant on *any* observatory; and if it were, *Malebolge* errs rather in excess than in defect of equipment, for the purposes of illustration and example.

I have alluded to methods of using the transit which may not be familiar to all my astronomical readers. It is to be understood that these apply to second-class or supplemental

observatories ; i.e. to those which *accept* the fundamental points in astronomy from the standard observatories.

To make a catalogue in right ascension of unknown stars, I should recommend an observer to take a long series of transits on any fine night in one position of the instrument, and to reobserve the same series on a following night with the instrument reversed. Allowing for clock *rate*, and taking the mean of the observed transits of each star on the two nights, the error of collimation will disappear from the result, and a series of equations may be formed, in which—

$$\text{Observed Transit} + m^* + n^* \times \tan. \text{ declin.} = \text{apparent R.A. of star,}$$

m and n being two constants, depending on clock error and instrumental error. The fundamental stars of the *Nautical Almanac* (especially if Polaris or δ Ursæ Minoris be of the number), afford an accurate determination of m and n , and a substitution of these values in the other equations, gives the apparent R.A. of each star. All that is assumed is, that the observed times are reduced by the clock rate to a given epoch each night, and that the *error of collimation* does not change in the interim. With the Liverpool transit the right ascension of a star would thus be determined in two favourable evenings, with a probable error not exceeding 0.05.

When *absolute* time is wanted, it is necessary to know the error of collimation. If there is a well-defined and distant mark near the horizon, this error can be corrected or measured by the micrometer ; but distant marks in the horizon are seldom well defined, and mostly invisible. The method I have usually followed is, to observe a set of well-known stars, including at least one star near the pole, then to reverse and observe a second and similar set, applying the level before and after reversing. The error of collimation is that which, when applied with different signs in the two series, compels them to yield the same clock error. Corrections for rate and level are previously made, and if the reversion is performed carefully, the azimuthal error will come out the same, or nearly so, in both series. This is not, however, necessary to secure success

when the observations are good, and the determining stars numerous and well known.

A method frequently followed, and which, with a manageable instrument on firm supports, is satisfactory enough, is to observe a slow moving star near the pole up to and over the middle wire, to reverse, and then to observe the same star over the remaining wires. When the several wires are reduced to the *mean*, and the result in each position corrected for the indication of the level, the factor for correcting collimation is equal to the difference of the two results divided by twice the secant of the star's declination, and its sign is shewn by the result. As the error of collimation very seldom changes, scarcely ever, I believe, in a well-made instrument, except from violence, it is sufficient to examine it from time to time, and when any discrepancy is perceived, to measure the error *de novo*. But in all cases where the time is required more closely than the tenth of a second, I prefer observing a set of stars in *each* position of the instrument, and so getting rid of the error of collimation in the final result. Whichever way be pursued, it is almost impossible to get wrong in the determination of the error of collimation more than three, four, or five-hundredths of a second of time, by one measurement.

I have admitted that doctors differ as to the best mode of measuring the collimation error. The Astronomer Royal holds that a mark, when it can be had, is rather better than the stars, and with very large or very small instruments he is certainly right. He tells me, moreover, that my own observations at Kingston last summer prove against my opinion. But the instrument there was small, the mark very favourably placed, and the weather singularly unpropitious to the celestial process, as very few stars could be seen, and those very irregularly, so that I am not quite convinced. Besides, the tendency to bias, and therefore to apparent consistency, is stronger when a mark is used, because you know how the bisection *should* look. Practically, the results are nearly identical; but the determination by the stars is almost always feasible in observing weather, that by a mark may often be impracticable for many days together. The Liverpool observer can try both, as both are at his command.

I have to offer some excuse for the long delay of this little publication ; other business interrupted me when the first excitement was upon me. Since then it has been with great reluctance that I could bring myself now and then to such a disagreeable and thankless task, and I have allowed any excuse to be valid. I wish I had been *more* to blame, but headaches, with other little ailments, have often indisposed me, and a meditation on Mr. Taylor's performances was any thing but a restorative. Perhaps it is as well that so trifling a subject should not have been broached during the war of railways and the stormy strife of politics. There may be some chance now of meeting with attention. The want of order and frequent repetitions have been forced on me by the form of publication, the nature of my subject, and the necessity of letting Mr. Taylor speak for himself.

CORRESPONDENCE,

&c.

To make the following Correspondence intelligible, I must premise a short account of the Liverpool Observatory. Within the last few years it had been frequently remarked that an observatory on some scale or other was almost necessary for this important port. It was, I believe, publicly suggested or proposed more than once prior to the meeting of the British Association at Liverpool, and at that meeting the subject was brought prominently forward by some of the leading members. Without entering into particulars, I may state that a committee was appointed by the town-council to establish an observatory, that this committee consulted several competent persons as to the site, equipment, and scope of such an institution, and that, agreeably to the information which they received, they caused the observatory to be built, and appointed Mr. Hartnup to be the director.

That Liverpool ought to possess an observatory seems not to be disputed; but, while the Observatory Committee and the scientific gentlemen consulted wished to have a modest and economical establishment, principally directed towards the improvement of navigation, and, secondarily, to purposes purely astronomical, Mr. John Taylor maintained, as he says, that a "noble observatory" on a "national scale," under the "patronage of the Lords of the Admiralty," was more befitting. It does not appear that any serious attention was paid to these suggestions.

I first saw the Liverpool Observatory in the spring of last year. I was there twice in the course of the summer and autumn, and convinced myself by *actually observing* that true time may be got with the transit there as well as at Greenwich, Cambridge, or Oxford. Having, therefore, considerable acquaintance with the subject, feeling the importance of the institution, and knowing the interest which the Royal Astronomical Society took in the director (who had been our assistant-secretary,) and in every thing which relates to the advancement of science, I wrote a paragraph for the Report of the Council, which

were called in question. The said five lines, which were to be a complete vindication of the Observatory Committee of the Liverpool Town Council, are as follows :—

“The Observatory is admirably situated for this purpose (*the giving true time to the great port of Liverpool*), on the brink of the Mersey, at the entrance of the Waterloo Dock; the horizon is good, and infinitely better than could have been hoped for in the heart of a busy manufacturing town.”

So that, according to the writers of this Royal Astronomical Report, the proper situation for our Observatory is in the lowest point of land that can be found, surrounded by hills that cut off the true horizon, and where, in fact, there is no horizon at all, although, to be sure, when the trough of the river is filled with smoke and fog, which is commonly the case, there may seem to be a horizon at a few yards distance;—where the view is immediately interrupted by the masts of ships, by immense and high warehouses only 200 yards distant, and with the expanse of the sky from the north-by-east to the southern point constantly filled by the mass of the town and by the smoke constantly lodged over it;—where nor sun, nor moon, nor star is ever seen, or was ever seen, to rise or set, and where *no meridian line* can ever be drawn or determined; on “*the brink of the Mersey*” too, by their own confession, exposed, on a foundation of red sandstone rock, for ever cracking, to the furious attacks of the surge rolled in from the north-west by the storms so frequent here.

Such a situation for an observatory may be thought a good one by the Royal Astronomical Society of London, whose great learning may enable them to see things with eyes of peculiar perspicacity; but to other men the place will probably still appear a *condemned hole*. Nevertheless, if it prove “a comfortable house for the *possible observer*,” one useful purpose will, after all, be served, nor need his time pass the less pleasantly from the impossibility to see.

It is lucky when the house suits the householder. Some would fancy the brink of the Mersey, with the backing of the docks, ineligible as a residence for man, woman, or child, in respect to health; and, as to comfort, not unlike the position of a figure-head on the cutwater of a transatlantic steamer. They might also fancy that fog and sea spray, with the fumigation of passing steam-boats, would corrode metal and glass, tarnish specula, and cause vegetation to shoot between the achromatic plates; that the tremor from the winds, waves, rolling of carts, discharging and loading of cargoes, and the shock of cannonadings, to say nothing of the crumbling of the foundation from the trickling of water through the flaws of the sandstone rock, would render a re-adjustment of the instru-

ments constantly needed, lest the giving out the time of the day by drop of ball should become a mere farce, no more to be depended on for the knowledge of the true Greenwich mean time than the dictum of a railway mail-coach guard by the help of his post-office timepiece.

But stop: these are matters of transcendant science, on which no one should venture an opinion if he be not an

⁹⁹⁹ M.D., an ⁹⁹⁹ F.R.S., an ⁹⁹⁹ F.R.A.S.L., (mind, printer, to put the circulating dots,*) or, at all events, of some standing above Zero; and not like the poor Frenchman Piron,—

Qui ne fut rien,

Pas même A-CA-DE-MI-CI-EN!

T.

Liverpool, 1st April, 1845.

From R. Sheepshanks to the Editor of the Liverpool Times.

[The following reply to some observations which appeared in the *Mercury* was intended for that paper, but the conductors of that paper not having yet been able to find room for it, it has been handed to us for insertion.—*Editor.*] See page 27.

SIR,—One of your correspondents, under the signature of T., and date of the 1st of April, has found great fault with a sentence in the report of the Council of the Royal Astronomical Society, which refers, in terms of approval, to the site of the Liverpool Observatory. Though the columns of a newspaper are not the proper place for a discussion which must inevitably be dull, if not unintelligible, to most of your readers, yet as that portion of the report was furnished by me, and its *correctness* has been called in question, I must beg of you to insert the following remarks in reply.

The main purpose of the Liverpool Observatory, and the proper business of the observer, is to get, keep, and communicate true Greenwich time to the great port of Liverpool. Will your correspondent say distinctly, that true Greenwich time has not been well got, well kept, and faithfully communicated? and, if he does say so, will he produce some proof of his assertion?

Is the situation conveniently situated for chronometer-makers, ship-owners, and captains? Is the drop ball widely visible? Can a better or more convenient site be pointed out in Liverpool (for near Liverpool and its port the Observatory, to accomplish its purpose, must necessarily be), at the disposal of the

* The point of the joke not visible. It is not safe to jest in an *unknown* language.

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* The point of the joke not visible. It is not safe to jest in an *unknown* language.

Corporation, and not interfering with other and perhaps more pressing matters? I will not affirm that a better place cannot be found, but I was most agreeably surprised, on my first visit, to find one so little objectionable. If T. should think this letter demands an answer, I beg that he will speak, and that definitely, in answer to these queries; in legal phrase, I wish issue to be taken or offered. I know of no large town, not actually on the sea-shore, where so favourable a situation could reasonably be expected.

In answer to some of T.'s not very pertinent objections, let me assure him that the disciples of Cassini were the last persons who ever dreamed of drawing or determining a meridian line for exact astronomical purposes: further, that there is no obstacle to drawing such a line, that I can see; but it would be a useless trouble and expense. The meridian of a transit observer is the mean wire of his instrument.* Again, observation† of the rising and setting of the heavenly bodies, though it formed an essential portion of Babylonish and early Egyptian astronomy, went out of fashion, I suspect, before the extinction of the Ptolemies. Lastly, it is scarcely ever practicable to make a satisfactory observation within ten degrees of the horizon, and never advisable to do so when it can be avoided. T.'s objections on these points rest on a less solid basis than the red sandstone of the trough of the Mersey, however severely that may be beaten and shattered by wind or tide.

T.'s description of the other local and atmospheric difficulties seem to me to be inaccurate, or uncertain, or grossly exaggerated. I have no faith in the long list of possible contingencies which, like horrible shadows, haunt T.'s imagination, and ride it nightmare fashion. An investigation of results (which the Observatory books will supply), and of the methods employed (which only requires a modicum of spherics and algebra, added to a little patience and good temper), will easily shew T. whether he is right or wrong in his random guesses. If Mr. Hartnup will permit, I hereby very cordially invite T. to a co-examination with me into the effects of the "crumbling foundation," the "shock of cannonadings," &c. &c. I hope to be in Liverpool in a few weeks, and shall be ready, with T.'s assistance, to ascertain whether the "readjustment of the instruments" has been "constantly needed." I doubt the fact. There are, luckily, no specula to be corroded; and if "vegetation should shoot between the achromatic plates of the object glass," which is not impossible, but unlikely, the lenses must be separated and wiped. Many of T.'s objections were charged, a few years ago, against the Edinburgh Observatory. My

* See page 50.

† Printed "observations" in the newspaper.

excellent friend, the late professor, found, nevertheless, no difficulty in making most admirable observations, and I trust Mr. Hartnup will follow Professor Henderson's example.

I have nowhere said that the situation of the Observatory was a *convenient one for the observer*. I did not indeed think it necessary to call attention to a few grievances which, slight as they are, are worth T.'s list twice over. There is no car-road, for instance, to the doorway at any time, and at high water there is no road at all. There is a little worry from the seamen's cries at high water, and from the town noise when the wind is unfavourable, but no shock or tremor; the communication is cut off by the sluice. A loud beat to the equatoreal journeyman will remedy this, the principal Observatory grievance, the only one in fact which I have felt in observing. T. objects to, and probably misunderstands, my use of the word "horizon." I believe the expression,* though incorrect, would be generally intelligible to astronomers. I meant briefly to express the fact, that a large and useful portion of the sky was open to the Observatory. Now to the north, where it is most limited, stars can be seen forty-five degrees below the pole. To the south a *Columbæ* (S. D. $34^{\circ} 9'$) has been observed. Hence almost every star which rises above the horizon of Liverpool, and all which would be visible if the horizon were perfectly clear, can be observed with the transit. The sky is not so open for the equatoreal, but still is satisfactory, especially on the best quarters, south and west. As the equatoreal instrument is destined to occupy the leisure which the observer may have after his regular business, he is sure of finding sufficient employment, as scarcely one phenomenon in fifty will be lost for want of sky room.

That there is more smoke at Liverpool than is pleasant may be allowed, but smoke does not often interrupt transit observations. It is less easy to form a correct opinion of the proper climate and site for an Observatory, than to call the estuary of the Mersey hard names. Piazzi of Palermo declared that Greenwich was "the paradise of practical astronomers." The late Mr. Troughton thought Fleet Street; Sir J. South held, if I mistake not, the Borough, to be more favourable to observation than Greenwich. My little Observatory in London, which would have obtained a far lower place than the Liverpool Malebolge in T.'s astronomical Inferno, yet shewed me stars as steady as Greenwich. I have no reason to believe (but rather the contrary) that the atmosphere of the Liverpool Observatory is worse than the vicinity of the town. That it has failings, I dare say, and where is the English climate which is faultless and cloudless?

* "The horizon is good and infinitely better than," &c. See page 17.

I will conclude this long letter by reminding my "benevolent readers" that T.'s objections are hypothetical, founded on no actual experience, and, possibly, not drawn from much practical acquaintance with Observatories; that the site of the Liverpool Observatory, *in reference to its object*, has been approved of by the late President of the Royal Astronomical Society, the lamented Francis Baily, by the Astronomer Royal,* by Messrs. Dawes and Lassell, Hartnup, and myself, all, in our various degrees, experienced observers; that the true Greenwich time has been actually determined and communicated for about a year at Liverpool, by Mr. Hartnup, and the instrument used by different observers. Finally, that the longitude of Liverpool has been thereby accurately determined, the present very small uncertainty being independent of the Observatory altogether. With respect to the building as a residence, I have not heard that the situation is found to be unhealthy by the inhabitants, or that Mr. Hartnup thinks himself miserable because he is "situated like a figure-head on the cut-water of a transatlantic steamer," or aggrieved beyond endurance because there is no carriage-road to his door. I cannot help hoping that T. himself will, on consideration, allow that his zeal has outrun his discretion upon this hobby; and I am confident that, after he has joined me in a careful examination of the real state of the Observatory, foundations, &c. &c., he will also agree with me in thinking it a great step towards promoting the security of navigation and an honour to the great port of Liverpool. — I have the honour to be, &c. &c.

R. SHEEPSHANKS,

Fellow of Trinity College, Cambridge, and Foreign Secretary of the Royal Astronomical Society of London.

Reading, April, 8th, 1845.†

[In the very same newspaper which contained the above communication, Mr. Taylor inserted the following letter.]

From John Taylor to the Editor of the Liverpool Times.

SIR,—I have only this morning had communicated to me a copy of a letter, from the Rev. Mr. Sheepshanks to the Editors of the *Liverpool Mercury*, in reply to some letters which I wrote a few weeks ago, under the signature T., on the subject of the Liverpool Observatory.

Mr. Sheepshanks' letter is well deserving of a candid and a distinct answer on every particular point that he indicates, as well as of every further illustration and explanation that may

* The Astronomer Royal did not *recommend* the site.

† Printed in the *Liverpool Times* of April 22, see the Preface.

be needful to set the question at rest. That answer it is my intention to give, with every courtesy due on such an occasion. I cannot, from other avocations, draw up my reply before the middle of next week, when it will appear in the *Liverpool Mercury*.

As Mr. Sheepshanks has given his own name, I think it only just towards him to subscribe mine also.

I am, Sir,

Your most obedient Servant,

JOHN TAYLOR.

April 19, 1845.

From John Taylor, to the Editors of the Liverpool Mercury.

"I doubt na, 'frien', you think ye're nae sheep-shank,
 Since you have got a *Secretary's* rank!
 But gin ye've read one half as much as me—
 Though faith that day, I doubt, ye'll never see—
 There'll be, if that date come, I'll wad a boddle,
 Some fewer whigmeleeries in your noddle."

BURNS (*cum licentiâ poetæ*).

GENTLEMEN,—A letter from the Rev. Mr. R. Sheepshanks, Fellow of Trinity College, Cambridge, and Foreign Secretary of the Royal Astronomical Society of London, has been published in the *Liverpool Times* of this day. The letter is in answer to the few observations under the signature T. respecting the site of the Liverpool Observatory, which you were so good as to publish for me in your newspaper of the 4th instant.

Mr. Sheepshanks calls upon me to speak, and that definitely, in answer to certain queries, on which, he says, he wishes issue to be joined. With your leave I will hasten to satisfy his desires.

"The main purpose of the present Liverpool Observatory, and the proper business of the observer, may be (according to the views of its founders) to get, keep, and communicate true Greenwich time;" but the confining the uses of an observatory (if it is to be called the Liverpool Observatory) to so limited a purpose, is the very thing to which I, for one, have from the first objected. The Liverpool Observatory ought to be on a much greater scale as to equipment, and as to the number of officers appointed to it. The charge, too, for its establishment and support, ought to be defrayed out of the Dock-rates, and not to depend on shabby starvation grants from the purse of the Liverpool Corporation. It should be a national, and not a municipal affair.—*See the Report of the Commissioners of Municipal Inquiry, published in 1832.*

I have to remark, also, that even the limited object of "true Greenwich time" cannot, in my belief, be regularly and constantly obtained by means of an observatory placed where the present one is, owing to the defect of its foundation and the faultiness of its position, although the time may be fortuitously hit off, and for some while pass as correct. That is my belief; but on this point, it seems, I have Mr. Sheepshanks for an opponent, and to work we must therefore go.

Answer to query first:—I cannot say whether "Greenwich time is well got, well kept, and faithfully communicated, or not;" neither can Mr. Sheepshanks, for another observatory would be required in Liverpool to determine the fact. Mr. Sheepshanks should not have made so unreasonable a demand. I am afraid he entertains too favourable an opinion of the abilities of his poor humble servant. It shews, nevertheless, how important it is that the observatory should have had every advantage of site, as well as of construction and equipment.

Answer to query second:—The Observatory is *not* "conveniently situated for chronometer-makers, shipowners, and captains."

I believe there is no chronometer-maker's shop nearer than Castle Street, which is about a mile off; although there may be places of safe deposit for seamen's watches (kept by their relatives, *videlicet*, their uncles), *Judæorum delubra*.

The shipowners, and the persons called the ships' husbands, live, nearly all, one, two, or more miles distant; and the ship captains mostly lodge or live with *wives*, whose dwellings cannot very readily be ascertained.

To the third query, about the *ball*, since that moveable has got such hold on Mr. Sheepshanks' mind (pretty much in the same degree, it would seem, as the moveables promised by King Richard got possession of the mind of the unfortunate Duke of Buckingham,) the answer is, that it is not widely visible, except from the river, where few ships lie, and where none lie that can help it.

To the fourth query:—"A better and a more convenient site for the Observatory, and one not interfering with other more pressing matters," could easily have been found—much more easily found than the money could now be found to build the Observatory over again. As to its being at the disposal of the Corporation, it is not, nor ever was, a matter to be admitted with propriety into the discussion. All the Corporation needed to have done in the affair was, to keep their money in their purse, and their paws off the concern.

And now for Mr. Sheepshanks' "other observations."

I have to choose between Mr. Sheepshanks and the following authorities, as to the *necessity* of determining and drawing

accurately a meridian line, in order to place the transit instrument rightly; namely, Cassini, Lemonnier, Lalande, Franceur, Biot, Delambre, and, in fact, every writer of any reputation that ever wrote upon the subject. Mr. Sheepshanks may set them all at defiance, but, until he fairly shovels them out of the way, I, and I suspect other folk, will trust to them. "The Meridian Line," says Lalande, "is of *great use* in astronomy, in geography, in gnomonics; all those sciences suppose that those who profess them know how to trace it exactly; it is this which has caused different astronomers to give the *greatest* care, and take the *greatest* pains, to describe it with the *greatest* precision." Farther:

"Some distinct point is marked on the horizon, on which the line of the telescope (the transit instrument) bears; this *terrestrial* object, placed in the meridian, serves to detect, if the instrument be deranged; to replace it in the meridian, in case of accident; to correct, if thought proper, at each observation, the small inequalities which heat may have caused, or, at least, to enable us to compute them in our observations." So says Lalande (in article 2606), and so says Biot; and so says every body that knows any thing about the matter, and will be candid. Now to describe a meridian line, or to set up in Malbolge a terrestrial object or meta, such as that now described, is impossible. If Mr. Sheepshanks were to attempt to draw such a line there, southward, he would not go many yards before he would fall into the river: and if he tried it northward, he would soon be in the dock.

It seems that Mr. Sheepshanks' definition of the horizon is, "a large and useful portion of the sky;" a very indefinite definition; so I will leave it, as being incomprehensible by me.

Mr. Sheepshanks says, "observations of the rising and setting of the heavenly bodies, though *IT* formed an essential portion of Babylonish and Egyptian astronomy, went out of fashion, I suspect, before the extinction of the Ptolemies."

Did it? What does Delambre say? He says (Lesson xii. No. 28), "The meridian passages of the planets interest only astronomers; but they are necessary for the calculation of the risings and settings, which *interest more particularly the public.*"

Shall we trust to Delambre or to Mr. Sheepshanks? I'll stick by Delambre. Besides, if we are to give up the true horizon (not the incomprehensible), what will become of observations of the planets, Venus and Mercury in particular; and of the eclipses of the sun or moon, when rising or setting; or of comets, when near the sun, with their flaming tails, one of which nobody could see here lately on account of the smoke of the town, the fog of the river, and the height of the Cheshire

hills? We cannot give up the tails; we will rather give up Malebolge.

In addition to the new definition of the horizon, Mr. Sheepshanks has favoured us with another new one of the meridian. He says, that "the meridian of a transit observer is the mean wire of his instrument." For this definition much thanks; hitherto they were supposed to be two separate things; and that the one was to be made to coincide with the other, or the observation would be false. All that trouble may now, according to Mr. Sheepshanks, be saved, seeing that the two are always one.

Yet, if it were so, what was the use of the first column of the right-hand page, headed Error of Collimation, in the astronomical observations published at Cambridge, by Mr. Airy, in the year 1833? The word collimation signifies the hitting of a mark; and, accordingly, an error of collimation is when the *line of vision passing through the middle wire does not coincide with the mark set up at a distance to identify the MERIDIAN LINE*. In that publication Mr. Airy says that "the values of the error of collimation were ascertained at Cambridge Observatory by means of THE SOUTHERN-MERIDIAN MARK, fixed on the steeple of Granchester Church, at the distance of about two and a half miles." I should like to know where the south meridian mark for the *Malebolge* transit instrument is? Perhaps Mr. Sheepshanks thinks that a dancing buoy, floating in the stream, will be good enough for Malebolge; and I think so too.

Mr. Sheepshanks seems to think it an advantage that no reflecting telescope can ever be kept in Malebolge, and that the achromatic plates can take no injury by the shooting of vegetation, on both which points I beg leave to differ from him.

If the Piazzini of Palermo thought Greenwich Hill "the Paradise of practical astronomers," he undoubtedly would have joined in my opinion of the infernal locality of the Liverpool Waterloo Dock Observatory.

But it is time to have done. My objections are not "hypothetical" nor "exaggerated;" they are facts, which have been repeatedly and plainly stated in the face of a population of 230,000 people, founded on knowledge of the locality, and on a long experience of the weather which prevails here. I will not take up more of the public time nor the space of your columns in rectifying the mistakes of Mr. Sheepshanks. My wish, my only wish, has been to forward the cause of science and the welfare of the community. Those objects are not with me "merely a hobby;" they are, and have been, through life, the passions of my soul.

I had brought the matter to a distinct offer on the part of the present Board of Admiralty to give the sanction and Parliamentary support of Government to the *establishment of a noble astronomical observatory, with schools attached to it, for teaching mathematics, astronomy, and navigation, and with examiners qualified to grant diplomas to the masters and mates of mercantile vessels*; the whole to be constructed and maintained on the ample, secure, and permanent foundation of a small percentage of the immense amount of dock-rates annually collected—a debt long past due from trade to science.

That measure was defeated by the wilful, and, I fear, by the self-interested obstinacy of those men who have, in its stead, set up the *thing* which is a present subject of praise for Mr. Sheepshanks, and will be one of contempt for future times.

JOHN TAYLOR.

Liverpool, April 22, 1845.

From R. Sheepshanks to the Editor of the Liverpool Times.

Athenæum, April 26, 1845.

SIR,—I regret that I should have to trouble you on a matter so little interesting to your general readers. The Editor of the *Mercury* twice declined inserting my reply to Mr. Taylor's comments; and, it seems, thought himself authorised to transmit the MS. copy to Mr. Taylor himself. Under these circumstances, common prudence forbids any application in that direction.—Your very obedient servant,

RICHARD SHEEPSHANKS.

The controversy between Mr. Taylor and myself is scarcely on equal terms, since he has, and I may not, have recourse to a peculiar phraseology. But however Mr. Taylor may write, *as from himself*, which is a matter of taste and education, I have a right to demand that he shall not so mix up his words with mine, as to leave persons in doubt as to what I have really said. In his anonymous strictures Mr. Taylor slipped his word* “possible” before my word “observer,” putting both in Italics, as if I had written them so. The intent was, I presume, either to convey a covert insinuation that observations are *not* possible, and therefore not made, which is untrue, or to mystify my words, which is unfair. I shall again have to allude to this unpleasant habit in Mr. T., and I wish once for all to hint

* Printed “vivid” in the newspaper.

were called in question. The said five lines, which were to be a complete vindication of the Observatory Committee of the Liverpool Town Council, are as follows :—

“The Observatory is admirably situated for this purpose (*the giving true time to the great port of Liverpool*), on the brink of the Mersey, at the entrance of the Waterloo Dock; the horizon is good, and infinitely better than could have been hoped for in the heart of a busy manufacturing town.”

So that, according to the writers of this Royal Astronomical Report, the proper situation for our Observatory is in the lowest point of land that can be found, surrounded by hills that cut off the true horizon, and where, in fact, there is no horizon at all, although, to be sure, when the trough of the river is filled with smoke and fog, which is commonly the case, there may seem to be a horizon at a few yards distance;—where the view is immediately interrupted by the masts of ships, by immense and high warehouses only 200 yards distant, and with the expanse of the sky from the north-by-east to the southern point constantly filled by the mass of the town and by the smoke constantly lodged over it;—where nor sun, nor moon, nor star is ever seen, or was ever seen, to rise or set, and where *no meridian line* can ever be drawn or determined; on “*the brink of the Mersey*” too, by their own confession, exposed, on a foundation of red sandstone rock, for ever cracking, to the furious attacks of the surge rolled in from the north-west by the storms so frequent here.

Such a situation for an observatory may be thought a good one by the Royal Astronomical Society of London, whose great learning may enable them to see things with eyes of peculiar perspicacity; but to other men the place will probably still appear a *condemned hole*. Nevertheless, if it prove “a comfortable house for the *possible observer*,” one useful purpose will, after all, be served, nor need his time pass the less pleasantly from the impossibility to see.

It is lucky when the house suits the householder. Some would fancy the brink of the Mersey, with the backing of the docks, ineligible as a residence for man, woman, or child, in respect to health; and, as to comfort, not unlike the position of a figure-head on the cutwater of a transatlantic steamer. They might also fancy that fog and sea spray, with the fumigation of passing steam-boats, would corrode metal and glass, tarnish specula, and cause vegetation to shoot between the achromatic plates; that the tremor from the winds, waves, rolling of carts, discharging and loading of cargoes, and the shock of cannonadings, to say nothing of the crumbling of the foundation from the trickling of water through the flaws of the sandstone rock, would render a re-adjustment of the instru-

ments constantly needed, lest the giving out the time of the day by drop of ball should become a mere farce, no more to be depended on for the knowledge of the true Greenwich mean time than the dictum of a railway mail-coach guard by the help of his post-office timepiece.

But stop: these are matters of transcendant science, on which no one should venture an opinion if he be not an

⁹⁹⁹M.D., an ⁹⁹⁹F.R.S., an ⁹⁹⁹F.R.A.S.L., (mind, printer, to put the circulating dots,*) or, at all events, of some standing above Zero; and not like the poor Frenchman Piron,—

Qui ne fut rien,

Pas même A-CA-DE-MI-CI-EN!

T.

Liverpool, 1st April, 1845.

From R. Sheepshanks to the Editor of the Liverpool Times.

[The following reply to some observations which appeared in the *Mercury* was intended for that paper, but the conductors of that paper not having yet been able to find room for it, it has been handed to us for insertion.—*Editor.*] See page 27.

SIR,—One of your correspondents, under the signature of T., and date of the 1st of April, has found great fault with a sentence in the report of the Council of the Royal Astronomical Society, which refers, in terms of approval, to the site of the Liverpool Observatory. Though the columns of a newspaper are not the proper place for a discussion which must inevitably be dull, if not unintelligible, to most of your readers, yet as that portion of the report was furnished by me, and its *correctness* has been called in question, I must beg of you to insert the following remarks in reply.

The main purpose of the Liverpool Observatory, and the proper business of the observer, is to get, keep, and communicate true Greenwich time to the great port of Liverpool. Will your correspondent say distinctly, that true Greenwich time has not been well got, well kept, and faithfully communicated? and, if he does say so, will he produce some proof of his assertion?

Is the situation conveniently situated for chronometer-makers, ship-owners, and captains? Is the drop ball widely visible? Can a better or more convenient site be pointed out in Liverpool (for near Liverpool and its port the Observatory, to accomplish its purpose, must necessarily be), at the disposal of the

* The point of the joke not visible. It is not safe to jest in an *unknown* language.

as a *definition* of horizon, he either commits a very gross mistake, or he does something much worse, and which I would rather not define.

If the risings and settings of stars interest the public of Liverpool, or of any other place, I admire their patriarchal and unsophisticated tastes; but I deny that these phenomena have belonged to the Observatory for centuries. This is to my mind implied by the quotation from Delambre.

The planets Venus and Mercury have for the last century been mostly observed in the meridian and not in the horizon, and though, with good equatoreal instruments, much *may* be done, out of the meridian, little *has* been done for their observation. The Astronomer Royal is constructing a special instrument for the moon, but this is an experiment of his own, and can only be carried into effect at such an establishment as his. Observations near the horizon are generally of small value; faint objects are invisible; all, ill defined. The comet to which Mr. Taylor, I believe, refers, could never have been observable at Liverpool with such means as the Observatory will possess. It was observed at Cambridge with the great equatoreal, and was just seen for an instant in London, by a very good telescope and observer. I wish Mr. T. joy of all the comets' *tails* he catches in the horizon. When the Liverpool equatoreal is mounted, it will be time to talk of what it can do. We must first settle the points in dispute about the *transit*, towards which we make slow progress.

In most observatories the meridian error is deduced from observations, treated in the manner which Mr. T. will understand more familiarly after the actual examination of the methods and verification of the requisite computations to which I have invited him. Distant and distinct marks, tolerably near the north and south points, would be useful and save a good deal of trouble, if such marks could be had at pleasure, but *distant and distinct objects in the horizon* are more easily demanded than found. I have seen none such. At most observatories an *optical* mark is adopted, not for a *meridian* but *collimating* mark,—such might be put up in the slits of the Liverpool Observatory, but I am not sure they would be worth the few pounds they would cost. By combining observations, made in reversed positions of the transit, the errors of meridian and collimation can be deduced with accuracy, by any person able to solve simple equations with two unknown quantities. I prefer this method myself, particularly with small instruments, which can be reversed easily; but doctors differ.

Mr. T. is in a hurry when he jumps to the conclusion that because I do not want a mark in the meridian, I therefore do *not* determine the meridian error and collimation error with

the utmost scruple. He will see how this is done from the Observatory books. There are two good ways of getting the same thing, and I prefer the stars to a mark. I could cite weighty authority on my side, weighty at least in Mr. T.'s judgment; but as the authority really is nought, I will rely on my own long practice, leaving every other person to his choice.

There is no room for a reflecting telescope (of course Mr. T. thinks of a large telescope, as a little one cannot be wanted, and would be easily protected) at the Liverpool Observatory. I did not say this was any "*advantage*," any more than the "shooting of vegetation" between the achromatic plates. I said (and I will trouble Mr. Taylor *not* to attribute *his* conclusions to *my* statements) that I did not anticipate such an evil, and that, if it did arise, there was an easy remedy.

If I, a stranger, had compared the magnificent estuary of the Mersey to the inner depths of Dante's hell, I should have expected to be called a very uncivil and impertinent person, if not worse. I have read Dante's description, and cannot for the life of me see any resemblance, except Fluellen's, that both begin with an M. But this is a parochial matter, which it does not become me to interfere in. I am satisfied by finding that Mr. Hartnup, who has enjoyed and observed in the pure and serene air of Blackheath, Greenwich, and London, can see and observe stars, and that right well, in the Liverpool *Malebolge*, since Mr. Taylor will have it so. This is a point which Mr. T. would do well to bear in mind. "There is no such thing as motion," said an ancient philosopher of Mr. Taylor's dogmatical school. "See, I can walk across the room," was the practical refutation; and perhaps this sort of proof will satisfy the people of Liverpool, though Mr. Taylor should still remain in his ancient belief.

From John Taylor to the Editors of the Liverpool Mercury.

GENTLEMEN,—In the *Liverpool Times*, of this day, I am again called to battle by the ecclesiastic trumpet of the Rev. Mr. Sheepshanks, and, with your permission, I will again turn out to the encounter.

In the gladiator shows of old Rome it was usual to clear off the mangled limbs, and strew fresh sand over the arena between the assaults; and I suppose, in this wordy war, the same rule must be observed, by preliminary references to former communications.

In the first place, then, I must trouble Mr. Sheepshanks to

eat his own words, namely, "That the disciples of Cassini were the last persons who ever dreamed of drawing or determining a meridian line for exact astronomical purposes."

I have proved the contrary by the undeniable evidence of Lalande, the greatest astronomical observer the world ever knew—of Mr. Airy, the Astronomer Royal—and, let me now add, of the late M. Bouvard, head astronomer at the Observatory of Paris, member of the Academy of Sciences, and superintendant of the publication of the *Connaissance des Temps*. See page 219 of the 133d volume of the *Connaissance des Temps*, where, in the preface to the Astronomical Observations made at the Observatory, M. Bouvard says, "The observations which we publish this year (1809) are made like those of preceding years, and with the same precautions; that is to say, by verifying *very often* the horizontal level of the telescope, the optical axis, and the *meridian direction*, by means of THE TWO MARKS (*mires*), the one to the north and the other to the south."

In the second place, he will have the goodness to eat also the words, "The French authorities he quotes were not transit observers." Lalande and Delambre were great transit observers, although Mr. Sheepshanks has kept himself so amazingly ignorant as not to know it. If Mr. Sheepshanks will take a cool walk to the British Museum Library, he will there, I have no doubt, find Lalande's French Celestial History, containing observations of 50,000 stars, made by himself and other French astronomers.

In the third place, Mr. Sheepshanks says that he used the words, "a large and useful portion of the sky," not as a definition, but as an *explanation*, of the word "horizon,"* in his former letter. Now, I find, in N. Bailey's English Dictionary (a very good one), the word "Definition (in the mathematics) an explanation of the terms used for explaining the thing treated of." The two words, "definition" and "explanation," are thus convertible at the pleasure of the party, and Mr. Sheepshanks gave a *definition*, as M. Jourdain spoke prose, without knowing it.

In the fourth place, full, distinct, and conclusive answers to Mr. Sheepshanks's several queries *were* given in my former letter (lines 33 to 69), and to these I beg to refer. It is tedious going over the same ground; and the long rigmarole of the last letter from Mr. Sheepshanks, is merely a repetition of fallacies already refuted, or miserably frivolous carplings at things of no signification, arising, doubtless, from an over-excited state of mind.

* Mr. Taylor should have said of the words "good horizon." See the Extract from the Report of the Council. (p. 17.)

Finally, Mr. Sheepshanks sets up his own authority against that of all preceding astronomers. He has not condescended to quote one authority in support of his doctrine, but seems to expect that he and his opinions, in a matter of dispute, shall be taken by the public at his own valuation. A bad bargain the public would have of them, say I; and so I conclude this my reply.

JOHN TAYLOR.

Liverpool, April 29, 1845.

From R. Sheepshanks to the Editor of the Liverpool Times.

SIR, — I congratulate you and your readers that the dispute between Mr. Taylor and myself is drawing to a close. Let me, however, before I untie Mr. Taylor's last knot, remind him, that he has not condescended to give a *direct* answer to *any* of my queries, and that referring to his former letter is merely a trick to delude persons too lazy to make the reference.

He has not pointed out a better situation for the Observatory. He has not verified one fault which he has found with the Observatory. He has not accepted or refused my offer of *proving* to him, *on the spot*, by reference to the *documents, instruments, computations, &c. &c.*, that *in each and every of his statements he has said the thing which is not.*

Having made this progress with Mr. John Taylor, now let me follow him into his own arena, which, oddly enough, is as bad a bit of battle-ground as he could have chosen — the *meridian line*. I am afraid I must begin by disentangling Mr. Taylor's ideas, in order to make all clear, which must be my excuse for being tedious.

"A meridian line, for exact astronomical purposes," consists of two parts, a small hole in the vault or wall of a very solid building, and a line distinctly drawn along the horizontal floor in the direction of the meridian. This line must pass, when continued, through the plumb line which hangs from the centre of the hole. The sun shining through the hole casts a bright spot on the floor. This cumbersome and inaccurate apparatus (I am comparing it with modern instruments and methods) is sometimes called a *gnomon*, for one of its principal objects was to determine the sun's altitude, sometimes a *meridian line*, when used for observing the meridian passage of the sun. It is clearly this meridian line Mr. Taylor thought of, when he objected to me that, in drawing it, I should fall into the Mersey or the docks.

Being pressed for time, I gave a short answer to this part of Mr. Taylor's letter, simply because it was the most absurd.

The meridian line, such as I have just described, never got into use as an astronomical instrument at all in England, and very little elsewhere. It was never used at Greenwich, an Observatory now a century and a half old, nor at Cambridge, nor Oxford, nor Edinburgh. I do not recollect to have ever seen one in any Observatory I have visited, though perhaps one may exist somewhere in the Observatory of Paris, set up by Dominic Cassini or his school. Almost the only meridian line which has furnished useful astronomical results is that established by Dominic Cassini, nearly two hundred years ago, in the Church of San Petronio at Bologna. A hundred years since, Lemonier drew a similar line in St. Sulpice, at Paris, with a lens in the hole, for the express purpose of examining the permanence of the obliquity of the ecliptic, and in consequence of this silly choice of astronomical means he was about the last person who doubted the decrease of that element, though he was convinced at last, — an example which I hope Mr. Taylor will follow. I give Delambre's reflections on these two observers, and their methods, for Mr. Taylor's edification, and I quote the original, as he and I shall have a few words before parting on the subject of his free translations.

“Admirens on plutôt félicitons Cassini d'avoir su tirer un parti aussi avantageux de son gnomon, qui l'a conduit à de meilleures tables du Soleil et à une théorie nouvelle et plus exacte des réfractions ; mais il faudra bien se garder dorénavant de recourir à de pareils moyens.”—*Astronomie Moderne*, vol. ii. p. 728.

Of Lemonier and his gnomon, Delambre says, “Au reste, il (Lemonier) ne croit pas que les gnomons soient d'ailleurs d'aucune utilité en Astronomie, et réellement on ne voit pas qu'il ait fait du sien aucun autre usage.”—*Astronomie au dix-huitième Siècle*, p. 180. At page 406 of the latter work Delambre says, “Il y a grande apparence qu'on ne fera plus de gnomons nouveaux.” Will our astronomical Rip van Winkle “stick to his Delambre” now? Will he give me one fact, one observation admitted into tables since the time of Dominic Cassini, which rests on this indispensable instrument. So much for the meridian line on the Taylorian system, such as will, no doubt, adorn his “noble observatory.”

On a smaller scale, such as can be drawn wherever there is a flat surface, one could be drawn, if wanted, at the Liverpool Observatory, provided the sun ever shines on “Malebolge.” A flagstone and an upright rod with a hole at top is “ample verge and space enough” for this more useful, though more humble, instrument. If the weather should be fine, I am good-natured enough to set up Mr. Taylor with this primitive observatory (he finding materials), and he can then enjoy himself

in repeating the observations of Pytheas and the predecessors of Eratosthenes; an occupation congenial to his tastes and acquirements, more innocent than that of libelling the Observatory and climate of his (native?) town, and, I may venture to add, far more advantageous to "the cause of science and the welfare of the community," which he says (and we are bound to believe) are the mainsprings which actuate him.

This smaller meridian line was frequently wanted by the French observers of the last century, some of whom used quadrants, sextants, and sectors, which had no azimuthal circle, or stop, to shew when they were in the meridian. Such were the instruments of La Caille, the sectors of the academicians, &c., and this is what Lalande means when he speaks of a meridian line for usual observatory purposes. But no such rickety and imperfect instruments, or such coarse modes of adjustment, ever got into our English Observatories. From Flamsteed downwards, the mural arc, quadrants, &c., were placed in the meridian as nearly as might be, and the error of plane or position determined by equal altitudes, and, latterly, by comparisons with the transit instrument. For the last fifty years, quadrants and meridian lines of verification have passed into oblivion, even in France. Mr. Taylor's nap has lasted over the reign of the *circle of Borda*, and he is not fully awake yet to the absurdity of the position he has taken up.

I thought every one had known that Lalande's reputation as a practical observer was moderate (is Mr. Taylor confounding him with La Caille?); but, such as it is, that it depended on *observations made with the mural quadrant*. Will Mr. Taylor look again, if he ever has looked before, at the *Histoire Céleste*? The observations of the 50,000 stars are all made with the mural quadrant, the errors of plane and position being, if I remember rightly, ascertained by equal altitudes with another instrument.

Lastly, under the same phrase, "meridian line," Mr. Taylor has included what Bouvard says of *meridian marks*, and the Astronomer Royal of marks *near* the meridian, which are used merely as *collimating* marks. I have already said that a collimating mark might be useful and save a little trouble in some cases; but I can shew Mr. Taylor, if he will only come to be convinced, that it is questionable whether a better error of collimation can be obtained by a terrestrial than by a celestial mark. This is the only subject on which I have given my opinion *as my own*; and as I have had a good deal of practice — as I have allowed that there is a difference of opinion, and, above all, as I have offered to submit my *modus operandi* to the keen, critical knowledge of Mr. Taylor, I cannot in fairness be said to expect "the public to take my opinions at my own valuation." The

meridian error is always determined in English Observatories by reference to stars near the pole; if Mr. Taylor doubts my authority, let him look at the annual volumes which issue from their directors. I knew a little of Bouvard and respected him greatly, especially as a computer; but his difference of longitude between Paris and Greenwich, by lunar transits, does not testify very strongly to the nice adjustment of the instrument at *one* of the places. We can do far better at "*Malebolge*." Here, however, Mr. Taylor is not much out. It is only about thirty-five years ago that my friend Mr. Pond set the fashion of observing Polaris, and knowledge of this late date is not to be expected from Mr. Taylor.

If Mr. Taylor really thinks *definition* and *explanation* mean the same thing, he meant no harm and I take no offence; but I trust he will be careful to let it be known that this is *his* notion of scientific language, not *mine*. With many thanks for your consideration, and an apology for my length, I have the honour to be your very obliged servant,

R. SHEEPSHANKS.

London, 3d May, 1845.

From John Taylor to the Editors of the Liverpool Mercury.

GENTLEMEN,—Another letter from the Rev. Mr. Sheepshanks, 164 lines long by eleven words broad (let us be accurate, for fear), has appeared in to-day's *Liverpool Times*, an awful length, streaming like tail cometic, or the beard of Sir Hudibras. Shall I, or shall I not, meddle with the horrid meteor, seize it, and shake it? Or, shall I let it pass unnoticed to fall, like other false lights, in blubber to the ground?

On the tradesman's principle of leaving no account unsettled, it may be better to bestow a few words upon it, in the charitable hope that the unhappy gentleman's perturbed spirit may be consigned to rest; and here they are.

Mr. Sheepshanks has, somehow or other, got his "ideas entangled" in a strange labyrinth. I never spoke of "any hole," small or large, "in the vault or wall of a very solid building," nor of "a line distinctly drawn along the horizontal floor, in the direction of the meridian."

I distinctly said that the meridian line was to be set forth by metæ or marks, placed north and south; and that if Mr. Sheepshanks attempted to set up such marks, south or north of Malebolge, he would soon be in the river, or else in the filth of the dock; for Malebolge happens to be surrounded by the two close at hand, as it is bounded a little farther off by

the high warehouses on one side, and by the Cheshire hills on the other. He could not get into the river if he kept in the room.

This one remark settles the first 128 lines of Mr. Sheepshanks's long-winded harangue, an harangue more easy for him to write, I am afraid, than for others to read.

He must get some other antagonist to take up his proposed statement, and tilt at the figment with all his might. It will be pleasant exercise for him, and I shall leave him unmolested to the quiet enjoyment of his pastime. And now for the rest of the epistle:—

In my letter which was published in the *Liverpool Mercury* of April 25 (line 33 to 69), I gave direct answers, distinct and plain as language could make them, to all the queries put by Mr. Sheepshanks, in the letter published by him in the *Liverpool Times* of April 22. To those two letters I again refer; and I call upon every man who has read, or who may read, those queries, and those direct, full, plain, and distinct answers, to form his own judgment of that which Mr. Sheepshanks says, when he avers that “Mr. Taylor has not condescended to give a *direct* answer to *any* of my queries, and that referring to his former letter is merely a trick, to delude persons too lazy to make the reference.”

To make an Observatory complete, terrestrial marks are required to shew the meridian line. Terrestrial marks, when once properly set up, remain constantly true, and check the errors of the transit instrument; which cannot be done by any *celestial marks whatever*.

Celestial marks depend on four points in the heavens, the equatoreal poles and the poles of the ecliptic. These four points are in yearly, daily, hourly, and momentary motion; and, from the *very nature of things*, the rates of these several motions never can be precisely ascertained. By algebraic assumptions, approximate values for them may be obtained; but these require to be finally tested by reference to the transits over the meridian line, as determined by the terrestrial marks. Without this all would be vague.

It is one of the many defects in the situation of Malebolge, that it admits of no terrestrial marks, such as have been described; and it is a defect which the eye of an astronomer ought at once to have detected. But there are two pursuits; the one, astronomical science; and the other, astronomical jobbing: with the latter, *time-taking* is often a mere sham; for the former, the *time-taking* must be of a very different sort.

When I designated as “noble” the establishment of an Observatory such as received the sanction and the proffered support of the Board of Admiralty, I trust that I did not

make an improper use of the English language; although, in the esteem of Mr. Sheepshanks, the authority of the Government of Great Britain may, like that of Lalande and of the other high astronomical authorities cited by me, be of no consideration.

I never proposed myself as the one to choose the proper site for the Observatory, nor to regulate the schools for mathematics, navigation, and astronomy, which were proposed to be attached to it. One thing I did maintain, long before Mr. Sheepshanks, probably, ever heard of the matter, that it ought not to be put into Malebolge.

I used every exertion early to divert the contrivers of that despicable establishment from their wretched plan. I had no private ends in view, and they knew it. They persisted, and thereby prevented a great public benefit; and Mr. Sheepshanks stands forward as the vindicator of those men who have inflicted a grievous injury on commerce, on science, and on the reputation of the country.

From Gosport to Gosport, all round the island of Great Britain, there was only one public Observatory, that of Edinburgh, near the sea-coast; and now they have set up a pepper-box in Malebolge, poor in equipment, and bad in situation, in cruel mockery of the public wants.

As to Mr. Sheepshanks's repeated and re-repeated irrelative insinuations about my "tastes and acquirements," my statements and arguments have been directed to the common sense of those who may have read my communications. The "good nature" of Mr. Sheepshanks shall not be tried by me; when I wish for further instruction, I will apply to some one whom I can believe to be duly qualified.

JOHN TAYLOR.

Liverpool, 6th May, 1845.

From R. Sheepshanks to the Editor of the Liverpool Times.

"Nihil tetigit quod non vitiauit."—*Goldsmith's Epitaph applied.*

"He made a mess of all he meddled with."—*Ditto freely rendered.*

SIR,—I admit that Mr. Taylor did not say one word *descriptive* of a *meridian line*, and for good reasons. He lumped in that phrase four different matters which he left me to develope. It is one of the hardships of contending with such persons that their crude and unlicked assumptions are first to be analysed, and then confuted. The "hole" and the "wall" and "the horizontal line" form the only kind of meridian line affected by the Docks and the Mersey. Mr. Taylor,

having given up three quarters of his meridian line, sticks by the fourth, viz., the “metæ or marks placed *north* and *south*,” which he attempts to defend by something which he calls argument, and I, something else. The fact which I have stated is, that in all British public observatories *celestial marks are used and alone used* for determining the *meridian* error; the *terrestrial* or *optical* marks being confined to the determination of *collimation*, for which they are useful and convenient, but not necessary, in my opinion. To meet this remark, Mr. Taylor merely expands his own assertion. I am quite satisfied, that, like “the three tailors of Tooley Street,” he should be “*unanimously of opinion*,” that all but he are in the wrong.

I repeat that Mr. Taylor does not know the meaning of a reply, if he flatters himself that he has answered *any* one of my queries; and he undervalues, I hope, the sense of his townsmen, if he expects them to believe that the way he has shuffled out of my invitation to verify his assertions, is anything but a confession of incompetence and inaccuracy. To state what is absurdly untrue, and then to skulk from inquiry, is a far greater fault than to know nothing of algebra and astronomy, and they are good things in their way.

I had fully resolved to introduce no irrelative or new matter in my reply to Mr. Taylor, but he has a *second time* stated that his “noble observatory” received the approbation of the Admiralty. On better authority than his I should have wondered, but perhaps believed this almost incredible statement. I may however say, *on my own personal knowledge*, that their lordships neither are, nor profess to be, authorities in astronomy. When they want information generally, they apply, as the Liverpool authorities did, to the Astronomer Royal; to regulate the Royal Observatory they employ also, very wisely, a board of visitors, of which I have been an active member for nearly twenty years. I see, therefore, no impropriety in my questioning the competency of their lordships to decide on the merits of a “noble observatory” at Liverpool, seeing they delegate the direction of Greenwich to other persons, myself among the number. I believe they eat white bait at the Crown and Sceptre periodically, but I am unable to connect this important fact with the observatory or with astronomy. I should like some proof of their “sanction,” and I disregard their scientific authority.

So ends my defence of the report of the Council of the Royal Astronomical Society, as far as it touches on the Liverpool Observatory. That Mr. Taylor has been *cowed* out of any attempt to establish the truth of his charges—(no man ever shewed the white-feather more decidedly); that every authority he has appealed to has been inapplicable, or turned out to be

adverse; and that he is totally ignorant of the aim, means, and management of an observatory, must be plain to any man of middling capacity, who has taken the trouble to watch his lame defence, or still more impotent and ignoble retreat. I have a few words more to say in vindication of the *tone* I have adopted towards this adversary, which I hope will be understood by my readers, as they are said, in all sincerity.

If Mr. Taylor had shewn the courage and fairness of a seeker after truth, if he had accepted my offer of examining the observatory and its records, and done his best to understand the subject, I would have spared him all exposure, whatever might have been the profoundness of his ignorance, or the dulness of his capacity. If his head "be made of penetrable stuff," I could, one way or other, have shewn him that he was in a delusion, and that poor "*Malebolge*," with its faults and inconveniences, was still as good an observatory, *for its proposed object*, as need be. There is no difficulty in teaching a child to get time to two or three-tenths of a second with a little practice, or to make the necessary computations; but Mr. Taylor took fright without taking warning, and left me no choice but to treat him according to his folly, though with the usual profitable result of washing bricks and blackamoors, so far as he personally is concerned.

I was as well acquainted with the author of the letter signed T. before he avowed his name, as afterwards. There is only one such man in a million, and if the stage had not been Liverpool, I could have sworn to the "sweet Roman hand" with perfect certainty. I must now shew, in justice to myself, and for the satisfaction of others, that my "repeated and re-repeated insinuations" about Mr. Taylor's "tastes and acquirements" are not incorrect, and that I have a very considerable acquaintance with them. I leave it to any man of common sense to say whether these insinuations are "irrelative" or not, for, be it remarked, *every statement to which Mr. Taylor adheres depends on his sole assertion*. Hence an inquiry into Mr. T.'s fitness to write on astronomical subjects is really to the purpose *here*, though in itself as idle a matter as may be.

My knowledge of Mr. Taylor, of his temper, talents, and acquirements, is of some standing. He first, I believe, burst upon the world in a letter to the *London Times*, on the subject of Halley's comet; and though he has occasionally used higher seasoning since, neither he nor any one else has ever surpassed this "scholar's prize" in absurdity. I give a short statement of a part of the contents of a letter inserted in the *Times*, December 3, 1835:—After rebuking the editor of the *Nautical Almanac* for calculating the places of the comet in

an elliptic orbit, he roundly asserts that a series of parabolic sectors is the proper mode of representing its path, prophesies that the comet will, after perihelion, bolt from the course prescribed, and, lastly, confounds in a breath "the method of least squares" with "squaring the circle," and identifies both "with the sieve of the Danaids!!!"*

I have found a key to a part of his mistake, which is illustrative of his "capacity." He had read, somehow, part of the *Système du Monde* (*Liv. 2, chap. 5*) in which La Place says, analogy leads us to suppose that comets move in ellipses, but as it is almost impossible to determine the major axis at one apparition, the motion is calculated as if the orbit were a parabola; which mode he goes on to explain. Now Mr. Taylor did not see, certainly did not understand, that the *periodic time*, and *consequently the major axis*, of Halley's comet being well known (there had been many apparitions), it was out of the parabolic category altogether. If he had drunk a little deeper, and proceeded as far as *Liv. 4, chap. 4*, of the noble work he quotes so glibly, he would have stumbled on this heading, "Concerning the perturbations of the *elliptic motions of comets*." It would be irony to recommend the careful perusal of the *Système du Monde* and Airy's *Gravitation* to Mr. Taylor. He does not understand the simplest statement, and jumps at a conclusion with the rapidity of a heaven-born genius. Perhaps he may begin to suspect that this royal road to science is less safe than easy, and that these matters are out of the domain of mere unaided "common sense." It may be as well to mention here, though not for the edification of Mr. Taylor, that Gauss's method *will* give the nature and diameter of the conic section in which a comet moves, at one apparition; see *Theoria Motus*. It will be an argument better suited to his conviction, to tell him that the most successful computer of comets we have now in England, Mr. J. R. Hind, assures me that he employs Gauss's method, and deduces the major axis of a comet moving in an ellipse to his great satisfaction.

To Taylor, "the prophet of evil," dame Nature gave the following reply from one of her most darling children: "Pray tell Mr. Stratford (with my hearty regards) that his beautiful Ephemeris seems to be *quite* as good as observation itself. It is really delightful to run down the columns of it, with the places deduced by actual observation."—*Letter from Sir J. F. W. Herschel, Mem. Royal Astronomical Society*, vol. x. p. 325.

The "method of least squares" has less to do with "squaring the circle" than Tenterden steeple with the Goodwin Sands. If any one wishes to know what this "method" is,

* See Appendix, No. 1.

I refer him either to La Place's *Essai Philosophique sur les Probabilités*, section "On the Application of the Calculus to Natural Philosophy," or to De Morgan's *Essay on Probabilities*, chap. 7, particularly page 155.* The relation of either "method" to the "sieve of the Danaids" I leave to the tenants of St. Luke's or Bedlam. Margites "knew every thing† and all badly:" my adversary "knows very little;" but I will back him against Homer's butt for knowing that little "a great deal worse."

My next acquaintance with Mr. Taylor's readings in science was three years later, when he took upon himself to tell Messrs. Dawes and Lassell what they saw, and what they ought to see in their telescopes; rather a cool assumption in a man who never made an observation, or looked through a telescope, for a useful purpose. I have just re-read Mr. Taylor's letters with feelings of disgust. They consist of gross ignorance and stupid misstatement, put forward with the coarsest insolence. It seems a part of this man's nature to misunderstand every thing he reads or hears of, and then flatly and flippantly to contradict the experience of well-informed and sober-minded persons. For instance, he denies that stars present any discs in telescopes. This is his *cheval de bataille* in the dispute. Now, every man who has looked through a good telescope, with even moderate magnifying powers, knows that bright stars *do* present very sensible discs; and if Mr. Taylor had read attentively and understandingly the authorities he quotes, he would have seen that this undoubted, every-day fact was never disputed before his letter appeared.‡

It is the *real* discs, and not the *apparent* discs, which are described as being mere points. It is by a *process of reasoning*, and thus *correcting the evidence of the senses*, that we are made acquainted with the extreme smallness of the *real* star, however brilliant it may appear. Horrocks, Lalande, and La

* I am not myself deeply versed in mathematics, and Mr. Taylor is, I am pretty sure, innocent of all such vanities; I have, therefore, referred to popular works as best suited to our talents. I cannot, however, refrain from quoting a passage out of Gauss's *Theoria Motus*, 1809, p. 6. After speaking of the previous necessity of employing parabolic orbits, he says, "We must except the celebrated comet of Halley, which, (though) describing a very oblong ellipse, (yet) having been observed many times at its return to perihelion, has (thereby) disclosed to us its periodic time. The major axis being thus known, the computation of the remaining elements is scarcely more difficult than finding a parabolic orbit." This was *before* the publication of his memoir. "Now," he continues, "the orbit of a celestial body can be computed sufficiently nearly, without any hypothesis, from good observations during a few days." Alas, for the astronomer of the *Liverpool Mercury*!

† I should have said "much," but I quoted from memory.

‡ See Appendix, No. 2.

Place, all rely on the argument derived from the instantaneous extinction of the bright stars when occulted by the moon. Mr. Taylor may, perhaps, discover this now, if he will look to the original authorities. To convince the most incredulous of my readers, I will first refer to the best of all authorities, Sir W. Herschel. In the *Phil. Trans.* for 1782, there are, between pages 93 and 178, four papers by this unrivalled observer, of inestimable value. At page 90 he states that the disc of α *Lyræ*, viewed with different powers, assumed the appearances of *Figs. 2, 3, 4, 5*, of *Tab. 4*, p. 110 (nice, round, sixpenny discs). At p. 102, he mentions that different powers alter these apparent diameters; and at p. 122, he says that *fig. 6*, in the same plate, represents *Castor*, seen with a power of 460. "The vacancy (between the stars) near two diameters of the larger star." Gentlemen who have dabbled a little in astronomy know that Sir William frequently estimated the distance of two close stars in terms of the diameter of the larger one. I refer Mr. Taylor to p. 99. "Those (stars) that are extremely near each other may be estimated by the eye in the measures of their own apparent diameters." Were these *diameters* mere *points*? Secondly, to Arago's exquisite "*Analysis of the Life and Works of Sir W. Herschel*," in the *Annuaire* for 1842, p. 360, to the chapter headed "*Diamètres apparents, diamètres corrigés, grandeurs réelles des étoiles*." Thirdly, to the following passage from the first English authority on practical optics:—"The image of a star will not be a point, but a bright circle surrounded by a series of bright rings. The angular diameter of these will depend on nothing but the aperture of the telescope, and will be inversely as the aperture." (*Airy on the Diffraction of an Object Glass with circular Aperture. Cambridge Phil. Trans.* vol. v. p. 287.) Lastly to the heavens themselves, though our Sidrophel has a natural antipathy to matters of fact and experience. If Mr. Taylor can, after verifying these references, read his letter in the *Liverpool Times* of December 17, 1838, without wincing, I admire his powers of moral insensibility.

I now come to the most ludicrous, though the most pardonable, of Mr. Taylor's extravaganzas, and one to which you, Mr. Editor, are an innocent party. In the *Liverpool Times* of August 2d, 1842, there appeared, under the signature of T. and with the heading "Astronomy and Photography at Rome," a notice of a memoir relating to sundry observations made at the Collegio Romano, by Father De Vico and his assistants. In an author like Mr. Taylor, it is scarcely worth while to reprehend any but the grossest blunders. I will, however, remark that this memoir is *not* the "precursor of a series," since two previous numbers had appeared; that "the mass of lunar

and planetary observations, made with micrometrical accuracy," consist, when examined, of a few observations, not remarkable for novelty or precision; that the contents of the memoir, though creditable, do *not* bear out the assertion "that the Observatory of the Roman College is henceforth to take place in the first rank of European scientific establishments;" and, finally, that the discoveries therein divulged have no pretension "to give a new starting point for the science of astronomy." Unhappily for Mr. Taylor, he undertook to announce, along with these minor flourishes, "a discovery of the highest importance to astronomical research" in the following terms:—*

PHOTOGRAPHICAL DELINEATION OF NEBULÆ.

* * * "Fortunately the Roman astronomers have hit on a means effectually to prevent future mistakes of vision or delineation. They have brought the Daguerreotype to bear on the object, and throwing the photographic image of the nebula and its stars on a lithographic stone, have, by an ingenious invention of the Signor Rondoni, which is still kept secret, fixed it there. From that stone they have been able to take impressions on paper, unlimited in number, of singular beauty, and of perfect precision; each star, each filmy nebulous streak, faithfully depicting its own position. * * * Altogether it is a discovery of the highest importance to astronomical research."

Shall I be believed when I say that this pompous announcement is all moonshine, that the discovery concerns lithography much, but astronomy little; that Mr. Taylor, having probably little acquaintance with Italian, and being certainly ignorant of the principles of mechanics, optics, and the Daguerreotype, has totally misunderstood the original? De Vico states very simply and clearly (see Memoir, p. 27) that the lithographer Rondoni has discovered a way of throwing a drawing on stone by the Daguerreotype (*i.e.* substituting a prepared stone for Daguerre's silver plate and Talbot's sensitive paper) such, that impressions can be taken from the stone by the ordinary lithographic process. Two specimens are given of the results of this process (which possibly may be of use in the arts), on the first of which Signor Rondoni has written

Fr. Rondoni dis. dal vero e trasportò il disegno in pietra col Dagerrotipo.

This I translate, "F. Rondoni drew from nature, and transferred the drawing to stone with the Daguerrotype." *Dal vero* "from the true," is the phrase used by artists to shew that the design is not a copy, but from the thing itself.

* See Appendix, No. 3.

I do not ask Mr. Taylor, in his own expressive language, to the unsubstantial but savoury repast of "eating his own words:" it would be like Grumio's offer of "the mustard without the beef." If, in his mortification and humiliation, our translator would vow to learn a little of the language and subject-matter of the author whom he honours by "doing him into English," it would be more creditable to himself, and more agreeable and profitable to his readers.

There are gross and perpetual blunders in this and in every portion of Mr. Taylor's writings which I have seen, too numerous to notice now. If he is not sick of the controversy, I may, perhaps, under another and more permanent form, give him a chance of being known as first in the first rank of the Marralls and Troublealls of astronomy; and if I should thus be forced to re-publish Mr. Taylor's contributions to science, *cum notis variorum*, I promise him a most unenviable notoriety. I have already said enough to weary your readers and exhaust your patience, but if I have succeeded in throwing even a partial light on this "palpable obscure," this "great unknowing," I am repaid. No man, who can put two and two together, and is willing to examine the dispute, can come to any conclusion but one, that Mr. Taylor's return of scientific and literary attainments must be *nil*. With many thanks to you for your kindness, I beg to subscribe myself, your obliged townsman and very obedient servant,

R. SHEEPSHANKS.

Royal Astronomical Society, May 10, 1845.

From John Taylor to the Editors of the Liverpool Mercury.

GENTLEMEN,—Three feet and three inches more of scurrilous abuse from Mr. Sheepshanks, in addition to the five feet six inches before published by him, have appeared in to-day's *Liverpool Times*,—a wild production of frantic malice, ranging from ten years back to the present hour, and referring to matters totally distinct from the point at issue. With such a disputant I decline further controversy.

His obloquy I hold in supreme contempt; and I set his threats of vengeance, by future libels, at utter defiance.

Of the main public question I have stated the facts; and having given my opinions, with their reasons, fully upon them; to those I refer.

Yours, &c.

JOHN TAYLOR.

Liverpool, May 13, 1845.

From R. Sheepshanks to the Editor of the Liverpool Times.

SIR,— Mr. Taylor's last letter is satisfactory, and perfectly conclusive on the merits of our dispute. That he has not been able to detect error or oversight in "eight feet nine inches" of "scurrilous abuse," "ranging from ten years back to the present hour," forms some presumption that the "production" is not "wild" and the "malice" not wholly "frantic." My madness seems to have had more method in it than the lucid intervals of my courteous respondent, when he was propounding his original and singular views concerning the orbit of Halley's comet, the physical constitution of Encke's comet, and the important discovery of the photographic delineation of nebulae.

Leaving to Mr. Taylor his exclusive supremacy in that branch of our mother tongue, which is said to be most purely spoken in the academy of Billingsgate, I assure him that the only "threat" I have addressed to him, the only "vengeance" or "future libel" which I contemplate, is that of printing this correspondence, with some extracts from his former contributions to science, and, perhaps, a few notes, critical and explanatory. The unhandsome conduct of the editors of the *Mercury*, and his perverseness and obstinacy in repeating what is manifestly untrue, viz., that he has answered my queries, "stated facts," and "given reasons," have left me no choice. When the letters on both sides and the original documents I have quoted are fairly laid before the public, if Mr. Taylor's name and pretensions be not "an argument for mirth" so long as he is remembered, I shall turn out as silly a prophet as I have shewn him to be.

I write now principally to correct an erroneous impression which Mr. Taylor's bold asseverations and my caution may have given of the site of "Malebolge," and also to state some matters which I have ascertained since my arrival here. And, first, of the "meridian line."

At the southern extremity of the meridian, and at about three miles distance, there is a large house, which supplies a number of well-defined marks pretty near the centre wire,—marks quite as well defined as those of the Observatories of Greenwich and Cambridge. I have satisfied myself by actual trial, that the collimation factor can be thereby ascertained to about one-twentieth or one-thirtieth of a second of time. So much I am bound to say for the mark; though I still prefer determining the value of this correction by comparing observations of the stars in reversed positions of the transit.

That this house and its chimney-pots furnish a meridian

mark of at least average accuracy, appears from this. In the last twelve months, Mr. Hartnup has touched his azimuthal screws but once; and he then brought the telescope into the meridian within a few hundredths of a second by reference to a stack of chimneys. Let not Mr. Taylor triumph upon this instance of the utility of a mark. The *correctness* of the instrument was ascertained by *Polaris*, according to the practice of all observing astronomers, the mark merely sparing a little fiddling trial and error of the adjusting screws.

As to the ball, I have four times observed it, and compared my chronometer afterwards with the clock. The average mistake in lowering the ball and my estimation of the time, was only one-tenth of a second; and, though chance had probably something to do with this close approximation, I believe two-tenths to be a fair estimate. Now the clock error in observing weather can rarely be wrong more than two-tenths of a second; and, in bad weather, will be as correct as two excellent clocks will carry on the time by rate. Mr. Taylor has said, that "no man, fit to command a ship, or to navigate by a chronometer, would ever think of trusting the safety of his vessel to so very uncertain an indication" as this ball. I should like to see the "*rara avis*" of a captain who would stand godfather to this notable saying. I would not trust so silly a boaster to cross the Mersey on a voyage of discovery to Birkenhead.

To shew the steadiness of the instrument (I believe it to be the strongest transit in the world), it will be enough to say that the collimation error has not sensibly varied for a full year, and that the changes of meridian error are within narrow limits, not having exceeded two or three-tenths of a second for the last two months.

As Mr. Taylor did not choose to investigate these matters himself (I believe he had *the best of all possible reasons* for declining my invitation), he must, in common fairness, adopt the above conclusions on my authority.

R. SHEEPSHANKS.

Liverpool Observatory, alias "Mulebolge,"
May 17, 1845.

I had written notes on Mr. Taylor's mistakes, but find them so numerous, and the errors so often repeated, that I have suppressed them. It will be sufficient to notice the essential points in the controversy.

Mr. Taylor has asserted his *belief*, that no reliance can be placed on the indications of the Liverpool transit. He has offered no *proof* of this beyond his own assertion; he has declined examining the records of the Observatory, which would have shewn whether he was right or wrong; and has not had the fairness to admit his mistake or his incompetence.

He has found fault with the site, but has pointed out none better (which is the real question), though repeatedly urged so to do.

He has heaped a great deal of abuse upon a very excellent institution, mainly, I believe, because he was not consulted about it, nor allowed to spend the public money on some costly whim of his own; and this he justifies by talking of his zeal for science and for the public good.

The only *scientific* point on which he has attempted to hold his ground is, a supposed defect in the present site; viz. that it does not afford the means of drawing a meridian line. I am willing to admit that, in my hurry, I did not develop Mr. Taylor's ideas clearly till the letter of May 3d. It is there shewn that Mr. Taylor confounded four different things, and quoted promiscuously authorities on each, applying to all the appellation of *meridian line*.

Now a *meridian line* has a meaning as definite in astronomy as a term can have. It is never used for a meridian *mark*, or collimating *mark*, but is a line drawn, as I have described in my letter, either on a large scale (when it is a part of a *gnomon*), or on a smaller scale, merely for a rough estimation of time, adjustment of buildings, piers, instruments, &c. There is plenty of room for a long line on the pier, but a *gnomon* would be of no real use; the smaller line can be drawn any where, but the object of such a line has been fulfilled long ago.

Mr. Taylor has attempted to retreat out of this difficulty, by saying he never meant such meridian lines as I have described. His authorities prove that he did. His first quotation from Lalande belongs to the ordinary meridian *line*, as the context shews. I cannot verify (for want of a reference) Mr. Taylor's quotation; and I cannot trust him for translating faithfully, or citing truly, but the passage clearly refers to the kind of meridian line thus described in Lalande, *Astronomie*, § 153; and, indeed, this may be the original to which he has alluded, such is the laxness of his citations:—

“La ligne méridienne est le premier fondement d'un observatoire: la plupart des observations supposent une excellente méridienne; car c'est sur les hauteurs prises dans le méridien, et sur les passages au méridien, que sont fondées toutes les théories astronomiques.”

This remark follows a description of the meridian *line*, such as can be drawn any where without astronomical instruments: Lalande's apparatus being “a small plate, with a pin-hole, raised on a stand seven or eight inches high.”

Mr. Taylor now repudiates all idea of a *gnomon*. Why did he then, for he must be the scientific friend of the *Liverpool Mercury*, send the following answer to a correspondent?—

“THE OBSERVATORY.—We are indebted to a scientific friend for the following answer to a correspondent:—‘The proposal of W. C. A., to have a meridian line traced for the port of Liverpool is one that naturally presents itself to all interested in navigation. By right such a line should be connected with the Observatory; but, unfortunately, the Observatory recently built, and now, as is said, in the course of equipment, is so placed that nothing of the kind can be done. The place chosen for it is an out-of-the-way corner, without room for any one useful astronomical operation. The area of the Exchange presents a tempting place for the tracing of a meridian line, and were that disgusting monstrosity, called Nelson's Monument, removed, as it ought to be, a *gnomon*, with its accompaniments, would form an appropriate part of the construction.’

“*Liverpool Mercury*, March 7, 1845.”

Again, if Mr. Taylor merely meant a meridian *mark*, and not a meridian *line*, how does his remark apply, that, on drawing such a line at the

Observatory, I must tumble into the river or the dock? A *mark* should be distinct, at a distance, and, of course, *visible*; but I never heard that there was to be a *pathway* from it to the telescope, nor did any one else, not even Mr. Taylor. When, therefore, Mr. Taylor says that in combating the meridian *line* I am tilting at a figment of my own, he lies, to speak mildly, under a mistake. It is, perhaps, an idle employment to explain his obscure meanings and shew their absurdity: but that a meridian *line* formed an item in his astronomical limbo is certain, both from his own language and from his authorities.

Immediately after quoting Lalande on the meridian *line*, he quotes him (§ 2606) on the meridian *mark*, evidently without any perception of the difference between the two. I give the passage with an extract from the preceding section:—"Dans un grand instrument des passages, l'axe étant bien horizontal, il suffit d'observer les passages d'une même étoile au-dessus et au-dessous du pôle, pour voir si la révolution diurne est partagée exactement par la moitié. On remarque alors dans l'horizon, sur un mur ou sur un clocher, quelque point distinct, sur lequel on aperçoit le fil de la lunette; cet objet terrestre, placé dans le méridien, sert à reconnoître si la lunette ne s'est point dérangée, à la remettre dans le méridien en cas d'accident, à corriger, si l'on veut, à chaque observation, les petites inégalités que la chaleur aura pu y causer, ou du moins à en tenir compte dans les observations." Having thus shewn how to set up a meridian mark correctly by observation of a circumpolar star, Lalande goes on to explain how the instrument may be placed in the meridian by the transits of two known stars differing considerably in declination (commonly called the method "by high and low stars"), *without any mark at all*.

Biot, *Astronomie Physique*, liv. i. chap. 5, *et seq.*, gets first a rough meridian line by equal altitudes of the sun, by which he places his transit: then adjusts by observation of a circumpolar star, and finally sets up *marks* for future reference. But he adds (note to section 79):—"On peut obtenir les mêmes vérifications par les seules observations astronomiques au moyen de la méthode indiquée, tome iii. page 130, *Additions*."

Perhaps Mr. Taylor may now find that Lalande, Biot, &c. are not quite so much on his side as he fancied them to be.

After citing Lalande on the meridian *mark*, Mr. Taylor quotes a much higher authority, Mr. Airy, but unluckily Mr. Airy refers to the *collimation error*, which is a different thing altogether. Mr. Taylor says, "The error of collimation is when the *line of vision passing through the middle wire does not coincide with the mark set up at a distance to identify the MERIDIAN LINE*." Now the error of collimation means no such thing. It is the angle which the optical axis of the telescope makes with the plane perpendicular to the axis of the pivots, and it may be discovered and corrected by observing a mark *any where near the horizon in reversed positions* of the instrument; the verification being analogous to the mode in which a carpenter or mason adjusts his *square*. If Mr. Taylor would learn the vocabulary of practical astronomy, he would not confound *collimation error* with *meridian error*, or a meridian *line* with a meridian *mark*. When there is a distinct *mark* near the north and south points, this will serve for ascertaining the collimation; and, indeed, there is no other use now made of terrestrial marks in the British public Observatories, though they go by the old name of *meridian marks*. But the collimation error can be as well got, in my judgment, by the stars, especially when the means of reversing are so manageable as at Liverpool, and the instrument itself so unchangeable.

Mr. Taylor quotes Bouvard, who mentions the use of meridian *marks* for collimation and meridian adjustment (in reply to my statement that Cassini and his school were the last persons who drew meridian *lines* for exact astronomical purposes), thus shewing that he was unenlightened as to the difference between *line* and *mark*. Accordingly, in my next letter, I

attempted to clear matters up, but with small success. Mr. Taylor gives up, indeed, his meridian *lines*, big and little, but still seems not to comprehend how a man can set up *marks* without actually *drawing lines* to them from the telescope. The Greenwich terrestrial mark is on the other side of the Thames. Maskelyne's old mark was, I think, on a chimney of the ranger's house. A deeper mist hangs over Mr. Taylor's intellect than ever settled on the trough of the Mersey, and there is no clearing off: he is "a *hazy philosopher* of about the tenth magnitude."

In his last attempt on the meridian line (p. 37), Mr. Taylor drops his authorities, and utters several solemn dicta, which are all his own. Every body but he, the writers he relies on most (Lalande, Delambre, Biot), all say that *you must set up your terrestrial marks by reference to pole-stars, especially Polaris*. The transit is adjusted when (the axis being horizontal, and the line of sight perpendicular to the axis) the circle described by turning the telescope round *passes through the pole*. Now the line of sight passes through the pole when the upper and lower transits of Polaris differ twelve hours. There are some corrections to be applied to the star, which are known to astronomers by the names of Precession, Nutation, Aberration, and to Mr. Taylor by the very "vague" term of "algebraic assumptions;" but the variations of these corrections are perfectly well known, ten times better than well enough for the purpose. His babble about the "ecliptic," "nature of things," and "momentary motion," is *nonsense* and mystification, *without any application* — mere *Taylorism*.

It is curious that, after all this ink shed, the Observatory is found to have a good south *meridian mark*, and a still better *collimating mark*, so that Mr. Taylor's objections are not merely silly in themselves, but not applicable. He is unlucky in his "guesses at truth."

I have already shewn how unfairly Mr. Taylor twisted my explanation of what I *meant* by a "good horizon" into a *definition* of the strict astronomical meaning of "horizon," simply. In the hurry of answering by return of post, I took no notice of his quibbling (p. 26) on the phrase, "that the observer's meridian is the mean wire of his transit." Now, when it is said that the *meridian* passage of a star is observed, all that is understood or intended is, that the star has been noted at its passage over the mean wire of the transit. This *sensible* meridian is then reduced to the *astronomical* meridian by applying corrections, which are obtained from the level, from reversing, and from the *observations themselves*. If we limit the use of the words meridian and horizon to the definition on which strict demonstration is founded, new words must be coined or tiresome periphrases will be required in practical astronomy. Mathematically speaking, there has never been an observation made in the meridian or in the horizon, just as there is no straight line and no right angle actually existing; but, as Mr. Taylor is the only person who cannot or will not understand ordinary scientific language, there is, perhaps, no pressing necessity for alteration.

I have said the authors quoted by Mr. Taylor were not transit observers, which, if my words are to be understood *literally*, that they *never* used a transit, is not correct. Lalande was not a good observer (see note p. 70), and Delambre and Biot are best known as *observers* with Borda's circle. Their merits, however, are of a higher kind than that of noting phenomena. The countryman of Flamsteed, Bradley, Maskelyne, Pond, and Airy; of Graham and Ramsden, Dollond and Troughton, need not have cited foreign authority on observatories or transit instruments. It is precisely the branch of science in which we are most distinguished, and the volumes which issue annually from Greenwich and Cambridge, Oxford and Edinburgh, are, I believe, unrivalled.

APPENDIX.

No. I.

HALLEY'S COMET.

THE return of Halley's Comet in 1835 exercised the skill of many geometers, among whom Rosenberger, Pontécoulant, and Lubbock, are particularly distinguished for the extent and success of their researches. The heavens were carefully watched in accordance with their predictions, and, to the delight of every one, the Comet made its appearance very nearly at the time and in the place where it was expected. As soon as a sufficient number of observations could be procured, the elements of the orbit were corrected, and fresh ephemerides published. Dr. Rosenberger, who had already made the motions of this body an especial subject of research, was most forward and most successful in these predictions; and in this country the superintendant of the *Nautical Almanac* applied the resources of his powerful establishment to the same interesting and important purpose. I believe I may assert, without being contradicted by any astronomer, that all possible assistance was afforded to them by this zealous and efficient officer. A few persons, indeed, unacquainted with the nature and difficulty of the problem, and not comprehending, moreover, the extent or nature of the assistance afforded, took upon themselves to criticise the superintendant and his ephemeris in the columns of the *London Times*. Among these would-be philosophers, Mr. John Taylor enrolled himself, and, though last in the field, he carried off the first prize of the *Dunciad*, against all competitors, named or anonymous.

I proceed to give an account of Mr. Taylor's performance, with extracts, and a few notes. His first letter is in the *Times* of Nov. 18, with the signature T., and is addressed to Lieut. Stratford, on his ephemeris of Halley's Comet. After some remarks on the discrepancies between Rosenberger's and Stratford's elements—*elliptic* elements be it remembered—the writer asks for “the three precise and authentic¹ observations, where and

¹ There are no such things in astronomy as *precise and authentic* observations, in Mr. Taylor's sense; if there were, a few observations would be as good as a large mass.

return is wanting, and always will be wanting, for the course varies.⁸ To designate the motion of the comet from place to place, there must be many sectors calculated of various parabolas, each sector founded on three good observations,⁹ the best observation of the preceding sector being made, when it can be effected, the first of that which is to follow.¹⁰ These separate sectors may be afterwards

for a small part of it, where it is the least disturbed and most strongly governed. I have looked, but in vain, to find what authority Mr. T. supposed he was following. La Caille, *Leçons Élémentaires*, § 795, says: "Lorsqu'on est certain du retour d'une comète, et qu'on a par conséquent le temps de sa révolution périodique, on doit en calculer les mouvements dans l'ellipse. * * * Nous dirons dans l'article suivant, comment on doit employer les observations, pour en conclure les élémens de la théorie d'une comète dans l'ellipse." He then gives as an instance the Comet of Halley, and uses Halley's elements.

⁸ The time of return was known well enough, *even before the Comet reappeared*, to make the calculations which depend on the *form* of the orbit, and "had not been wanting" for this purpose since the time of Halley. It was most accurately known at the time Mr. Taylor wrote; for the Comet had been observed for some weeks, and the perihelion passage, which is the chief difficulty, determined. That it never "will be wanting" is equally certain, unless men should desert the school of Newton and Laplace, and follow after Sir R. Phillips and Mr. John Taylor, a most improbable supposition. That "the course varies" is true, but inapplicable. These variations can be computed, and applied to correct the curve which the Comet would describe if undisturbed, as all astronomers know.

⁹ & ¹⁰ It is curious, that, while the superintendant of the N. A. in England and Rosenberger in Germany were busying themselves in correcting elements and predicting *future* places (an ephemeris is a prediction, and is intended to precede and facilitate observation and reduction), nay, had already succeeded in bringing the Comet within narrow bounds, the editor of the *Times* should admit the dreams of a writer who has so little acquaintance with the resources of astronomy as to recommend a series of parabolas, each of which is to express three places *already observed*. How were the *future* places to be predicted by this halting, dodging, waiting-on-Providence process? But, leaving this absurdity for Mr. T.'s consideration, is it possible to account for his blundering?

I have in my letter (p. 41) attributed the mistake to a hasty misunderstanding of a part of La Place's *Système du Monde* (lib. ii. chap. 5.) *De la figure des orbes des Comètes, et des lois de leur mouvement autour du Soleil*. I cite the text, which, though it contradicts Mr. Taylor, when understood, has still some sentences which seem to have led him on his false chase: I mark these last in Italics. "Le Soleil étant au foyer des orbes planétaires, il est naturel de le supposer pareillement au foyer des orbes des comètes. Mais ces astres disparaissant après s'être montrés pendant quelques mois au plus; leurs orbes, au lieu d'être presque circulaires comme ceux des planètes, sont très-alongés, et le soleil est fort voisin de la partie dans laquelle ils sont visibles. L'ellipse, au moyen des nuances qu'elle présente depuis le cercle jusqu'à la parabole, peut représenter ces orbes divers; l'analogie nous porte donc à mettre les comètes en mouvement dans des ellipses dont le soleil occupe un des foyers, et à les y faire mouvoir suivant les mêmes lois que les planètes, en sorte que les aires tracées par leurs rayons vecteurs, soient proportionnelles aux temps.

"Il est presque impossible de connaître la durée de la révolution d'une comète, et par conséquent le grand axe de son orbite, par les observations d'une seule de ses apparitions; on ne peut donc pas alors déterminer rigoureusement

pieced together, or fitted into a something, by the assumption of a great axis deduced from the interval of time which has elapsed between the comet's departure from the sun in 1759, and its return to the sun in 1835; and that piece of patchwork, or something formed by the parabolic sectors, may be set forth, and have the name of an ellipse given to it with perfect ease by any one, if probability and if accuracy be dispensed with.¹¹ But to labour at the discovery of any one ellipse or parabola, in the expectation thereby to exhibit accurately the motion of a revolving body, solicited by various

l'aire que trace son rayon vecteur dans un temps donné. Mais on doit considérer que la petite portion d'ellipse, décrite par la comète pendant son apparition, peut se confondre avec une parabole, et qu'ainsi l'on peut calculer son mouvement dans cet intervalle comme s'il était parabolique." La Place then shews that the area described by the radius vector of a body moving in a parabola $= \sqrt{2} \times$ area described by the radius vector of a planet, revolving at the perihelion distance. "On a ainsi le rapport du secteur de la comète, à celui de la planète fictive; et il est aisé par ce qui précède, d'avoir le rapport de ce secteur, à celui que trace dans le même temps, le rayon vecteur de la terre. On peut donc déterminer pour un instant quelconque, à partir de l'instant du passage de la comète par le périhélie, l'aire tracée par son rayon vecteur, et fixer sa position sur la parabole qu'elle est censée décrire." After stating the difficulties of determining the five elements from observation, La Place adds,— "Malgré ces difficultés, on est parvenu par diverses méthodes, à déterminer les éléments des orbites des comètes. Trois observations complètes sont plus que suffisantes pour cet objet: toutes les autres servent à confirmer l'exactitude de ces éléments, et la vérité de la théorie que nous venons d'exposer."

Taking the constitution of Mr. Taylor's mind, and the nature of his acquirements, into account, I think I can perceive that he had the foregoing passage in view when he wrote his letter. He overlooks the first part; skips the clause which limits what follows to comets at their first appearance; then by a process which baffles all understanding, mistakes the parabola, which is used under different circumstances as a convenient substitute for the ellipse, for a necessary curve, although the major axis is actually known; and, finally, misses the "confirmation," and, consequently, the correction, which other observations give to the three first employed.

I have looked at Lalande's chapter on Comets, and find nothing to lead me to suppose that it or Delambre's chapter on the same subject has fallen under Mr. Taylor's notice. The abridgements of Lalande and Delambre, and the *Leçons Élémentaires* of La Caille, give no hint of Mr. Taylor's new method, at least to my dull eyes. Does Mr. Taylor know the meaning of the words parabola and ellipse? His language is so loose, that I could almost suspect he does not; and then, with the further misunderstanding of Laplace, his error is accounted for. It is to be hoped the astronomers of the London *Times* and of the Liverpool *Mercury* are not fair specimens of editorial discrimination.

"It would be a waste of time to try to fathom the depth or to enlighten the darkness of this writer's ignorance. Every thing that he says cannot be done has been done; every thing that he proposes to be done is sheer nonsense. How would Mr. Taylor patch parabolas, having a common focus (I suppose he admits that the sun is the focus of each parabolic sector), so as to form a re-entering curve? If he had known the meaning of the words parabola and ellipse and how to put two ideas together, he could not have made such absurd blunders; if he had possessed a vestige of good taste, he would have "dispensed" with his pert concluding remark.

external and internal attractions in quick succession, as Halley's comet has been in the course of the present year, and even in this current month,¹² is worse than attempting to square the circle, the folly of which was typified by the ancient mythologists in the fable of the daughters of Danaus attempting to fill the sieve with water. The sieve is a circle divided, as well as it can be, into small squares, so that this fashionable method of 'least squares' is not quite so new as our modern calculators imagine. Like the parallelograms of Mr. Robert Owen, it will have its day, and, like them, be laid aside for some other antiquity under a new name.¹³ Were Mr. Stratford to give to the public parabolic sectors, calculated as before expressed, or furnish them with the fifty-six observations (and not 112, it seems) which he has judged to be trustworthy, he would do real service to science."¹⁴ But the immense calculations which he has enumerated are all labour spent in vain, as the result has shewn, and will further shew.¹⁵ His ephemeris may come near in one or two places, or for a short time; even Mr. Lubbock's track was apparently near the truth in the early parts of the comet's course after it crossed the ecliptic in its ascending node. But there was a woeful falling off when the time of trial came, in October;¹⁶ and the same will be the case with any elliptic orbit now formed and declared when the true places of the comet shall be found by observation in the spring of next year.¹⁷ Good luck as well as skill will be needed to come within a degree of its positions in March

¹² Mr. Taylor does not appear acquainted with the word *perturbation*, and has a very imperfect notion, if any, of the way in which the "*solicitations* by various external and internal attractions" affect a celestial body, and no notion whatever how the effects of these "*solicitations*" are to be calculated and applied. Perhaps, before he writes *ex cathedra* on this and similar subjects, he had better learn the A, B, C, of practical and physical astronomy.

¹³ If I had stumbled upon these speculations without notice, I should not have expected to find them dated from a residence so commonplace and sane, as 17 Bedford Street North, Abercromby Square, Liverpool.

¹⁴ If Mr. Stratford had given these parabolic sectors, he would have been unfit for his office. The observations themselves have most of them been long before the public, but I have not yet heard what use Mr. Taylor has made of them.

¹⁵ I have already quoted Sir J. F. W. Herschel's opinion (p. 29), formed *after* the Comet had passed the perihelion, and, therefore, when mistake was impossible. Will Mr. Taylor say Sir J. Herschel did not see the Comet, or could not observe it?

¹⁶ An attempt to be lively at Sir J. Lubbock's expense. It is almost impossible to calculate the perturbations of a comet's orbit with such a nicety as to predict the future perihelion passage *accurately* before its reappearance. As the motion of the comet near perihelion is very rapid, the neglect of a small term in the perturbations of the major axis may throw the Comet a good deal out of its predicted place, by causing a sensible error in the time of perihelion passage, though the *form* of the orbit is well known. This insolent remark, which is as ignorant as insolent, is addressed to a gentleman who devotes the scanty leisure of a "royal merchant" to the cultivation of severe science. Mr. Taylor has something more to learn than logic and astronomy, and something better.

¹⁷ So says Taylor the *prophet*; compare Sir J. F. W. Herschel the *astro-mer*, cited in my letter. (Page 41.)

and April.¹⁸ Such is my belief, and time will shew how far I speak without knowledge."¹⁹

The rest of Mr. Taylor's letter contains some vague speculations on the changes of the perihelion of the comet, its cause and consequences, which do not need any notice, as my readers will easily conceive.

No. II.

ENCKE'S COMET.

THIS comet, though insignificant in appearance, is perhaps the most interesting with which we are acquainted. The shortness of its period brings it frequently within notice, and gives a higher probability to its approaching, and consequently being *disturbed* by, the planets of our system. There seems little chance at present of getting a good approximation to the mass of Mercury by any other means: the mass of Jupiter, deduced from his influence on the comet, first proved the former mass was erroneous, and is found to accord with the result drawn from the latest observations of the periodic time and greatest elongation of the fourth satellite: and the apparently regular diminution of its periodic time has led Professor Encke to investigate the hypothesis of a *resisting medium spread through space*; a hypothesis which has now become matter of general

¹⁸ Luck has nothing to do with such operations as Lieutenant Stratford's. If good observations be treated in the method he has so well described, it is impossible to fail of success. Astronomy is not a "conjectural art," whatever the *scientific friend* of the *Liverpool Mercury* and the correspondent of the *London Times* may say.

¹⁹ Time has shewn, and that very decidedly, the quantum and nature of the knowledge with which Mr. Taylor has spoken; but failure does not seem to have improved either his understanding or his manners. He reminds me somewhat of the village-schoolmaster.

"In arguing, too, the parson own'd his skill,
For, *é'en though vanquished, he could argue still*;
While words of learned length, and thund'ring sound,
Amaz'd the gazing rustics rang'd around."

May I venture to continue the parallel?

"*But past is all his fame!*"

astronomical belief, though more time must elapse before this belief can pass into conviction. Its reappearance in 1838 was, therefore, impatiently looked for. Professor Boguslawski saw it at Breslaw, on August 20th; Encke at Berlin, on September 16th; Sir J. South, on September 21st; Mr. Dawes at Ormskirk, on September 27th. The observation of Boguslawski was called in question rather cavalierly by Sir J. South, in a letter in the *London Times* of October 3d. A letter from Encke, forwarded by Sir J. South to the *Times*, October 16th, expressed an opinion that Boguslawski had been deceived, and on that text Sir James further remarked that the object seen could not have been a nebula, as it was "possessed of motion," nor faint stars, as "these cometo-poietic stars moved also." In the *Times* of November 15th there is a letter from Sir James South, containing a reply from the astronomer of Breslaw, vindicating the observation of August 20th and explaining the means he took to avoid being deceived. This reply was admitted to be satisfactory by Sir James, who, indeed, said that he never meant to call the observation in question.¹

While Sir J. South was running a-muck in the *London Times*, (for Encke's Comet gave occasion to several erratic and eccentric proceedings on the part of the observer of Campden Hill,) Mr. Taylor was similarly employed in the *Liverpool papers*. It is already stated that Mr. Dawes saw the comet at Ormskirk on September 27th. There could be no doubt of identity, as it was nearly in its predicted place, and there is no nebula in that neighbourhood. Mr. Dawes says, "Its appearance is that of a hazy star of about the tenth magnitude."² (*Liverpool Times*, Oct. 2, 1838.)

¹ Without this explanation, Mr. Taylor's allusions to *Breslaw* would have been unintelligible. He evidently understood Sir J. South to mean that Boguslawski had never seen the comet at all, and so did every one else, though there is no express assertion to that effect in Sir James's letters.

² The whole of this tedious controversy depends upon the meaning of one simple phrase, "a hazy star." To my mind, "hazy" expresses the same notion as *νεφελωδής* and "nebulosa," and though Mr. Dawes's description is perhaps not so complete and graphic as to enable an artist to give a good likeness of the object, it is more than sufficient for identification. No one can suppose that what was seen was an ordinary star or planet, and it was something round and hazy. An astronomer who dabbles in antiquity—I should more properly, perhaps, speak of Mr. Taylor as an antiquary who dabbles in astronomy—ought to have considered that *star* is a general name, like *ἀστήρ* and *stella*, which requires an epithet to define its meaning, *fixed star*, *ἀστήρ ἀπλανής*, *stella fixa*, and that "*hazy star*" is good English for a comet.

In the *Phil. Trans.* 1791, p. 81, I find this passage from Sir W. Herschel, "Jan. 17, 1787. A star with a pretty strong milky nebulosity, equally diffused all around, the star is of about the ninth magnitude." A memo-

In the *Liverpool Albion* of October 29th, Mr. Taylor, under the signature T., after referring to "some erroneous observations made, or pretended to be made, at Rome and Breslaw," added, "Nor was the Ormskirk observation much better, when it described the comet as resembling a star of the ninth magnitude. This comet never was like a star of any magnitude whatever, and never will be; its appearance has always been that of a thin, filmy nebula."

I do not find that Mr. Dawes took any notice of this inaccurate and impertinent remark earlier than in a letter published in the *Liverpool Times* of December 4th. He first mentions the remarkable tenuity of the comet, as, although it passed *centrally* over a star of the seventh magnitude, on November 8, at 9^h 11^m 29^s G.M.T., "the brilliancy of the star was not lessened half a magnitude." He then gives a still more remarkable proof of its extreme tenuity :—

"Soon after I had marked the time when the centre of the most condensed part of the comet was on the star" (the star of the seventh magnitude above mentioned), "I observed occasionally a most minute point of light near the star; on the place of this, with reference to the larger star, I kept my eye, and soon became assured of its existence as a diminutive companion to it. I did not, however, anticipate the fact; that after the nebulosity of the comet had ceased to affect it, such was its minuteness that I could only just steadily keep it in view, and estimated its magnitude to be about the twelfth, or, according to the notation of Professor Struve, the eleventh, and of Sir John Herschel, the thirteenth. That the existence of such an object should be first discovered nearly through the centre of a comet is singular indeed. Its

random to the observation says, that "having but just begun, I suspected the glass to be covered with damp, or the eye out of order; but yet a star of the tenth or eleventh magnitude just north of it was free from the same appearance." So that this first of all observers could not distinguish between nebulosity and dampness on the glass. This nebulous or hazy star (the words are nearly synonymous) must have been very like Encke's Comet, only a little brighter. Sir J. South called the comet, at its first appearance, a *nebuloid body*; which is tolerably equivalent, astronomically speaking, to *hazy star*, though not quite such good English. (Nebuloid is a useless hybrid, for it means no more than nebulous, and has a Greek tail to a Latin head. The termination in abstract science is proper to *nouns* only, as spheroid, conoid, cycloid, rhomboid, &c. shew; the *adjective* form is spheroidal, &c. *Nepheloid* would be correct, but why have another word for *nebula*, which has got established? By "cometo-poeitic stars," Sir James meant a cluster of faint stars, which was mistaken for a comet, but it is not easy to interpret these playful and pregnant phrases.) That Sir James did not know how to observe this faint body, does not damage his evidence on a matter of eyesight, where he is more at home, and a very respectable witness. It is hard to believe that Mr. Taylor was really so dull as to disbelieve Mr. Dawes' observation because he *did* not understand the language in which it was conveyed.

position with relation to the larger star is about 10° or 15° south preceding, and distance in arc 20 or 30 seconds. Forty-five minutes after this observation the most condensed part of the comet passed a few seconds south of a star of the eleventh magnitude, which was frequently discernible during the transit. It seems, therefore, that a *cometary occultation* of a fixed star is almost an impossibility; so far, at least, as this specimen is concerned."

After some observations of the comet, Mr. Dawes proceeds to reply as follows to T.'s remarks on his preceding letter:—

"It might suffice to compare together my actual statement with this professed reference to it, to shew that the discrepancy which characterises the latter is only equalled by its inaccuracy. My words are,— 'There can be no doubt of the identity of the body, as there is no *nebula* (previously discovered, at least) in that neighbourhood with which it could be confounded. Its appearance is that of a *hazy* star of the *tenth* magnitude.' This description clearly shews that the body seen was *nebulous*, though considerably condensed, and more luminous in some part of it; and that its *quantity of light* was about equal to that of a star of the *tenth* (not the *ninth*) magnitude enveloped in *haze*: in which state, as any telescopic observer well knows, no stellar disc would be discernible. Surely, before writers (anonymous ones especially) indulge in remarks on the supposed inaccuracies or blundering pretensions of others, they ought to be a little more fair and exact themselves. I described the comet as it appeared to me through my instrument; and I defy T., or any one else, to find now any object answering to my description in the place laid down in my letter to you of September 29th.

"It is remarkable that the most condensed part of the comet is not coincident with the apparent centre of the nebulousity. It has always been observed by me to occupy a considerably eccentric position in the north following quadrant, being the side most distant from the sun. I am, yours," &c.

Mr. Taylor's letter in reply to Mr. Dawes, to which I have particularly called attention (page 42), appeared in the *Liverpool Albion* of December 10th. It will be for my readers to decide whether, in speaking of it so strongly, I have gone beyond what the occasion demanded:—

"To the Rev. W. R. Dawes, Dissenting Minister, Ormskirk.

"SIR,—In a letter published by you in the *Liverpool Times* of yesterday, you complain of 'a reference to your first observation of Encke's Comet on September 27th, which appeared in the letter of a correspondent of the *Albion* of October 29th, under the signature T.' You complain of a misrepresentation, which you state to consist in the wrong quotation of one phrase,—the words 'a star of the *ninth* magnitude' having been used instead of the words 'a star of the *tenth* magnitude,'—and with appropriate gravity you aver, that 'writers

(anonymous ones especially), before they indulge in remarks on the supposed inaccuracy or blundering pretensions of others, ought to be fair and exact themselves.' Your readers will, no doubt, be deeply thankful for this piece of valuable and new advice. But why be so prodigal of your treasures? Why leave yourself destitute in your care for others? It is generous, to be sure, but it is improvident. Had you cast but one charitable look on your own letter, you would have seen that your words were not 'a star of the tenth magnitude,' but 'a star of *about* the tenth magnitude,' of which the comet had the appearance: and, if you had consulted any of your familiar acquaintance, you might have found out that $9\frac{1}{2}$ is generally understood to be somewhere about 10; and that 9 is as near to $9\frac{1}{2}$ as 10 is.³ Why, then, so severe on an immaterial, and, obviously, an unintentional mistake, if it can, indeed, be called a mistake at all? Do take a little of your own counsel; it is very good counsel, and you will find yourself much the better for it.

"The remark made in the *Albion* that 'the Ormskirk observation was not much better than the observations made, or pretended to be made, in Rome and Breslaw,' you designate as 'flippant.' Just so does your neighbour the quack, who vends the Ormskirk cure for the hydrophobia, consider all those as 'flippant' who call his nostrum in question. Yet, notwithstanding all his and all your assumption, men will be found unwilling to surrender their right to form and to make public their opinions on such points of physics and of astronomy as may be brought before them. The question at issue, and from which you endeavour to divert the argument, is, whether you did or did not see Encke's Comet through your telescope on the night of September 27th? My belief is, that you did not see the comet, whatever else you might see: and my reason for that belief is, that you describe it as having 'the appearance of a star of about the tenth magnitude seen through a haze,' which appearance it never had, nor ever can have, through any good telescope, while it preserves its present physical constitution. Had you really seen the comet on September 27th, it would have presented itself to you as a faint filmy nebula, having hardly any concentration, with little or no radiance, and not, in the least, like any star, hazy or not hazy.⁴ When you say, that 'no stellar disc can be discerned when enveloped in a haze,' you make your own ignorance manifest. If a planet be discernible through a haze, its disc

³ This is what Mr. Taylor calls *reasoning*. Moreover, he drops the word "hazy," and drops it dishonestly.

⁴ Does Mr. Dawes's explanation of what he meant, and what every body but Mr. Taylor understood, by "hazy star of about the tenth magnitude," really differ from this tautologous description? Mr. Dawes's words imply the *roundness* of the comet, which Mr. Taylor's do not, as well as diffuseness, faintness, and some concentration. Besides, whatever might be Mr. Taylor's opinion previously, he continues his offensive and silly doubt *after* Mr. Dawes had explained what he meant by "hazy star,"—viz. "that it is nebulous, though considerably condensed." The addition of "not hazy" belongs to the penny-a-line style of smartness, and is an attempt to mislead.

will be magnified by the application of telescopic power, and a haze is the very thing that gives an apparent disc (a false one) to a fixed star.⁵ If a telescope, in a clear sky, make the disc of the fixed stars discernible, it, as well as those who trust in it, had better be employed in other investigations than in those of astronomy.⁶ *The pretension to*

* Mr. Dawes's words are, that "a stellar disc is not discernible in a star of the tenth magnitude enveloped in haze," which Mr. Taylor, as usual, quotes incorrectly. It is not easy to set such a disputant right in a few words, but I will try.

In a certain state of the atmosphere, bright objects are seen as a candle through a fog, or through ground glass, or through a window on which moisture is deposited. Simon Marius's description of the nebula of Andromeda is that it is like a candle seen through horn,—and a good description too. The effect of these obstructions is to dilute and diffuse the light, leaving no definite outline. Now, through a haze of this kind a star is a mere blotch, and as the diffused light is the illumination of particles in the atmosphere, the apparent size *is increased* by putting a higher magnifying power on the telescope, whatever the object may be, star or planet. When stars are called "hazy" or "nebulous," it implies that they are seen *as if* seen through such a medium. Mr. Dawes's words, so understood, seem perfectly correct.

But there is another and quite different state of the atmosphere,—when merely the *brightness* is subdued, as when the sun is viewed through a dark glass. In this case the definition of stars is improved in sharpness, their apparent discs are diminished, the coloured rings disappear, and circumstances are then most favourable for the measurements of those which are still sufficiently bright to be visible. This is also called haze, and is the state alluded to by Sir W. Herschel in the passage cited by Mr. Lassell, which I here reproduce. *Phil. Trans.* 1803, p. 224. "The air was very hazy but extremely calm. I had Arcturus in the field of view, and the haziness increasing." Sir William then refers to an accompanying plate, in which ten apparent magnitudes of Arcturus are drawn. "The last magnitude I saw it under could certainly not exceed two-tenths of a second, but was perhaps less than one." To say that haze, in any case, gives an *apparent disc* to a star is ridiculous. The cause that the *discs* of stars are *apparent* while their *true* magnitude is inappreciable seems to be twofold: from the physical nature of light (the effect of which is investigated by Mr. Airy in *Cam. Phil. Trans.* vol. v. p. 287), and also because the impression made by a bright object on the retina extends beyond the geometrical image. That portion of the apparent disc which depends on the first cause will evidently be magnified by increasing the magnifying power, while the other portion will, if any thing, decrease as the intrinsic brightness of the image becomes lower. Unfortunately, the experiments which would decide this question require considerable instrumental means and a very patient and conscientious observer. If Mr. Dawes or Mr. Lassell could be induced to undertake them, the business could not be in better hands.

⁶ The reader is requested to take particular notice of this sentence. It is the other leg on which Mr. Taylor halts. That "a hazy star of about the tenth magnitude" *cannot possibly be Encke's comet, though seen at the right time and in the right place*; that "a telescope which, in a clear sky, makes the disc of the fixed stars discernible" *is a worthless and ridiculous instrument*, are the two fundamental theses which this adventurous disputant has undertaken to support.

give the position of any point in the heavens, by means of such an instrument, in such hands, would be ridiculous.⁷

"I should not be thus plain in the matter were it not that your 'observations,' dated from an 'Observatory,' and registered as such in the pages of the *Nautical Almanac*, might mislead some unfortunate calculators, unaware of the weakness of that certificate. It is only fair to put such on their guard. As to your discovery of a very minute point of light, seen by you near a nameless, unidentified star (your description 'of 10° or 15° south preceding, and distance in arc 20 or 30 seconds,' being vague and somewhat difficult to understand); all that I shall say on the said point is, that it 'brings Breslaw to my recollection.'⁸

"T."

"*Liverpool, 4th December, 1838.*"

⁷ I notice this merely to shew that Mr. Taylor can scarcely speak *positively* to any astronomical fact without speaking *ignorantly*. Almost any telescope is *good* enough to give the *position* of a star or a comet in the heavens, if it will shew it at all: there are certain observations, those of double stars, for instance, which require exquisite telescopes, and those of nebulae and faint objects, demand plenty of light. There is not much difference (so far as the accuracy of the results) between a very good and a very moderate telescope, when the *position* of Encke's comet is to be fixed.

⁸ I here give a practical proof of Mr. Taylor's rashness in "making public his opinions on points of physics and astronomy." The *tabular* place of Encke's comet, at 9^h 11^m 29^s G.M.T., Nov. 8, 1838, is, when corrected according to Mr. Dawes's observations (see *Liverpool Times*, Dec. 4),—

R.A. 18^h 12^m 28^s. North Declinat. 48° 2' 59".

Now if Mr. Taylor will turn to Lalande's *Histoire Céleste*, p. 354, and will reduce to Jan. 1, 1838, the eighth star from the top (or the last star but two of those observed on April 18, 1790), he will find its *mean* place to be,—

R.A. 18^h 12^m 20^s. North Declinat. 48° 2' 57";

and I presume he will admit that the star is *identified*. The small corrections from tabular and mean to the apparent places (Mr. Taylor's "*algebraical assumptions*") are omitted in the calculation, which they do not materially alter.

Again, as to the small *companion*, which Mr. Dawes referred to its *principal* in the usual way, Mr. Taylor may be assured that the information supplied by the letter shews the relative appearance to be this:—

•

So that Mr. Dawes's description proves to be neither "vague," nor "difficult to understand," though Mr. Taylor, as usual, so confounds what is said of the two stars in his quotation as to shew his inability to comprehend it. In one respect, and one only, Mr. T. and I agree; viz., that "it brings Breslaw to our recollection." The astronomers of Ormskirk and Breslaw have each had their observations erroneously called in question on insufficient grounds.

It is desirable that the above-mentioned star should be carefully reobserved, as the central passage of the comet over it gives a better place than can be obtained by any other means. I shall scarcely be contradicted when I say this single observation outweighs in value all Mr. Taylor's astronomical lucubrations, even supposing them purified of their blunders and bad taste.

This extraordinary letter produced two replies, inserted in the *Liverpool Albion* of December 17th. The first from Mr. Dawes, who cited the authority of modern observers to prove that in the best telescopes, on the most favourable nights, and with high powers, the stars did shew discs. Thus, in a note to page 12 of *Phil. Trans.* 1824, part iii., Herschel and South say that, with the seven-foot telescope, "under favourable circumstances, with a power of 600, the discs of the two stars of η *Coronæ* and of σ *Coronæ*, and of ζ *Boötis* and of ζ *Orionis*, are shewn perfectly round, and as sharply defined as possible." At page 88 of the same memoir, it said that ζ *Orionis* was seen on a favourable night, "exquisitely defined, the division quite sharp and black, and the stars themselves like a shilling and sixpence side by side." Sir J. Herschel, using the same seven-foot telescope, says, *Mem. Roy. Ast. Soc.* vol. v. p. 76, note to 456, "the stars absolutely fixed, round, flat plates." Mr. Dawes then replies to T.'s difficulty about the identification of the star mentioned in his previous letter, that as the Greenwich time of the coincidence of the star with the comet was given (the errors of the ephemeris being also stated), there could be no difficulty in the matter.

The second letter of the same date is from Mr. W. Lassell, jun., who, after giving a short and clear *resumé* of the dispute, states that T.'s assertion, "if a telescope, in a clear sky, make the disc of the fixed stars discernible, it had better be otherwise employed than in astronomy," is contrary to experience and to the highest authority. He cites Sir W. Herschel, *Phil. Trans.* 1782, pp. 93 and 101; Herschel and South, *Phil. Trans.* 1824, part iii. p. 137; and further remarks, that the effect of a haze while observing with a good telescope may be to diminish the apparent discs, without interfering with their roundness or sharp definition,—see Sir W. Herschel, *Phil. Trans.* 1803, pp. 219 and 224. Mr. Lassell ends by shewing that T.'s complaint of vagueness in Mr. Dawes' assigned place of the small accompanying star in his letter, arose from ignorance of the language used by double-star observers, who define the place of the companion by its position and distance with respect to the larger star.

These answers from such authority ought to have shewn T. that he was quite wrong; but in a newspaper controversy on scientific subjects, the hardier asserter is pretty much on a par with the better reasoner, especially when the dispute is carried on in different papers. T. returned to the charge, in a letter to the *Liverpool Times* of Dec. 18th:—

"In the *Albion* paper of to-day there appear two long letters; from Mr. Dawes, of Ormskirk, and one from Mr. William Lassell,

jun., of this town. Any one who peruses those letters will be able to judge of the temper in which their writers discuss a scientific subject, and how far they conform to common civility. I do not think that it would become me to continue a controversy placed on such a footing; nor do I believe that Mr. Dawes, or Mr. William Lassell, jun., are either of them capable of understanding the mathematical truths on which the simple question (whether Mr. Dawes saw the comet on September 27, or rightly described what he did see, which was the only point put forward by me) depends. To them I have nothing to say. But there is something due to the public, and there is something due to myself, in so far as I may be known to any part of the public, that makes me appeal to your justice for the insertion of a few lines.

"Mr. Dawes says that he can discern the disc of a fixed star; now Sir John Herschel, in his *Treatise on Astronomy*, article 584, p. 374, distinctly says, 'that the discs of fixed stars cannot be discerned.' Fixed stars may be discerned as luminous points, varying in the intensity of light according to what is called their magnitude, but no real discernible or round surface, so as to compare one to a shilling and another to a sixpence, can be discerned, or appear, except it be through the imperfections of a telescope, or by the use of certain glasses, intended, for special purposes, to give an occasional false appearance to the star.⁹ My authorities for this assertion are Horrocks, Huyghens, Lalande, and Laplace. To give quotations from the works of these master-spirits in the science of astronomy would take up more time and space than I can bestow on this occasion, or than I can reasonably require from you in the columns of your paper. I have not Huyghens' works, nor those of Horrocks, this moment at hand; the articles in Lalande are No. 2784 to 2808: but one line from Laplace may suffice:—'The parallax of the fixed stars is invisible; seen in the most powerful telescopes their discs are reduced to luminous points: in that particular these stars differ from the planets, of which telescopes augment the apparent size.'—LAPLACE'S *System of the World*, chap. xi. book 1.

"If Mr. Dawes had seen the comet on September 27, he would have seen that its disc could be magnified by the increase of telescopic power, which a fixed star never could; and he, therefore, would never have described it like that to which it never did and never can bear resemblance, while it preserves its present constitution.¹⁰

"*Liverpool, Dec. 17, 1838.*"

"T."

⁹ What these "certain glasses," and what these "special purposes" are, I am anxious to learn, never having heard of such. Mr. Taylor is here beginning to shift his ground. The authority of the two Herschels and of South must have shewn him that his original assertion was altogether erroneous, and that *good telescopes*, with high powers, *do give discs to the stars in a clear sky*. The statement now is, that "the imperfections of a telescope" are the cause of apparent discs, which yet is not true. But if it were, Mr. Taylor is still bound to shew that Sir W. and Sir J. Herschel and Sir J. South, as well as Messrs. Dawes and Lassell, "had all been better employed in other investigations than in those of astronomy," and that their "pretensions" are "ridiculous," or to confess that he was mistaken.

¹⁰ I have already shewn that if, instead of Encke's comet, Mr. Dawes had

Mr. Lassell's reply is inserted in the *Liverpool Times* of December 25th. He first shews that the quotation from Sir J. Herschel, when complete and understood, does not bear the meaning given to it by T.; which is equally true of the quotation from Laplace, liv. i. chap. 13; for if by "points lumineux," mathematical points had been meant, Laplace would scarcely, in the following sentence, have talked of *proving* "la petitesse du diamètre apparent des étoiles," by the suddenness of their extinction by the moon. After a remark that, of the authorities quoted, Huyghens was, properly speaking, the only very high optical authority, and that his telescopes were not achromatic, he says:—

"I cannot pass over without remark the following sentence of T.'s letter: 'Nor do I believe that Mr. Dawes, or Mr. William Lassell, jun., are either of them capable of understanding the mathematical truths on which the simple question (whether Mr. Dawes saw the comet on September 27, or rightly described what he did see, which was the only point put forward by me) depends.' It would be quite as much to the point for T. to say that he did not believe I understood Chinese. No 'mathematical truth' is involved in the question, whether an object was or was not seen, nor even in its correct or erroneous description. It is easy to make use of scientific terms, but not so easy always to apply language according to its true import."¹¹

T.'s answer is in the *Liverpool Times* of January 1, 1839,—This is a curious instance how far a man may mistake a question, and how much twaddle may be written against an indisputable fact.

"THE DISCS OF THE FIXED STARS NOT DISCERNIBLE.

"The following authorities deny that the discs of the fixed stars can be discerned:—

"*Kepler's Epitome of Astronomy. Book IV. Part 1, at the end.*

"Skilful artificers deny that any quantity, as of a round body, is

really seen a star through a haze, it *would* have been magnified by the increase of telescopic power. A nebulous or hazy star would of course be magnified too, and both proportionally to the power employed. The authority of Sir W. Herschel, *Phil. Trans.* 1782, p. 102, proves that the apparent disc of a fixed star is also magnified, though not proportionally to the power (p. 74, note 17). I may add, that the proposal to ascertain whether such faint objects as Encke's comet are stars or comets, by Mr. Taylor's method, never could come into an observer's head.

"It is Mr. Taylor's habit, as well as Mr. Pecksniff's, "to use any word which occurs to him as having a good sound, and rounding a sentence well, without much care for its meaning." What does Mr. Taylor understand by the words "mathematical truths," "physical constitution of a comet," &c. ?

detected in a fixed star by means of telescopic observation ; but rather by how much more perfect the instrument, by so much the more the fixed stars are represented as mere points, from which the rays of light, like hairs, issue forth and are dispersed.'

" *Horrox's Posthumous Works. Dispute II. Chap. 4.*

" 'I have often in this year, 1637, compared the star Spica with Jupiter ; but Spica appeared much less than Jupiter, nor, truly, much greater than one of the satellites of Jupiter, if you disregard the rays that stream from it like hair. Wherefore, if the diameter of Jupiter be never greater than 50", as Kepler testifies, the diameters of the fixed stars will prove to be altogether insensible and impossible to be observed. Wherefore (to make an end) all geometrical measurement of the size of the fixed stars is perfectly frivolous, seeing that neither their distance nor their apparent semi-diameters can, unless by conjecture, be demonstrated ; which things being unknown, every road to this part of the measurement of the heavens is shut up.'

" *Huyghens' System of Saturn, page 540. Edition.*

" 'I have never been able to discern the diameter of the fixed stars as having any width, but only like very small points, as often as I have made use of glasses slightly dimmed with smoke to take away the radiancy. But by the advice of Hevelius, as given in his great work on the delineation of the moon, covering the object-glass, so that only a small opening was left, I saw them shewing some magnitude which I do not consider as really belonging to the stars, but as arising from a deception of sight.'

" *Gregory's Astronomy. Book III. Prop. 61.*

" 'The diameter of the brightest fixed stars cannot by any optical instrument yet invented, be made so big as to become sensible.'

" *Emerson's Astronomy. Section I. Problem 7.*

" 'It is remarkable that through the best telescopes, a fixed star only appears like a lucid point without any sensible magnitude.'

" *Lalande's Astronomy. No. 2808.*

" 'After having seen at what a prodigious distance the fixed stars must be, we shall not be astonished at the extreme smallness of their apparent diameters, and of the impossibility of our determining their absolute size and their true diameter.'

" *No. 2811.*

" 'If the diameter of a star was 1", and its annual parallax 1", the real diameter of the star would be equal to the radius of the earth's orbit, that is to say, thirty-four millions of leagues (ninety-six millions of miles).'

"La Place's System of the World. Book I. Chap. 11, of 2d Edition. Chap. 13 of 6th Edition, 1835.

" "Of the stars and of their movements.

" "The parallax of the stars is insensible ; their discs, seen in the most powerful telescopes, are reduced to luminous points ; in that these stars differ from the planets, of which the apparent size is augmented by telescopes.'

"Delambre's Astronomy. Lesson 2. No. 29.

" "We recognise the planets, first by their particular movements, and afterwards by their perceptible discs, for the most part round, which hinders their being confounded with the fixed stars.

"No. 30.

" "The stars, properly so called, are nothing but luminous points which are not magnified by telescopes, and which, on the contrary, appear less in telescopes than they do to the naked eye.'

"Sir J. F. Herschel's Treatise on Astronomy, No. 591, page 379.

" "Quitting, however, the region of speculation, and confining ourselves within the limits which we are sure are less than the truth, let us employ the negative knowledge we have obtained respecting the distances of the stars, to form some conformable estimate of their real magnitudes. Of this telescopes afford us no direct information. *The discs which good telescopes shew us of the stars are not real, but spurious ; a mere optical illusion.*'

"For the value of these authorities—they were all men who spent their whole lives in the study of astronomy, and in the constant observation of the stars ; and all of them, with the exception of Emerson and Gregory, have left numerous and highly valuable observations of the stars, recorded in their writings. Horrox, a native of Liverpool, was the first man who calculated and observed the transit of the planet Venus over the sun. Huyghens was the first man who measured time with accuracy, having applied the pendulum to clocks. He first determined, mathematically, the magnifying powers of telescopes. He was also the first to detect the true nature of Saturn's ring, and to discover the satellites of that planet. His observations were made with two excellent telescopes of 12 feet and of 23 feet focus, with a magnifying power of 100, of which a particular account is given in his works. Lalande was a skilful astronomer, and as skilful and diligent an observer from his early youth to his dying day, which happened in the year 1807, after a life of 76 years. He was, in fact, the greatest observer that the world has ever known. For sixteen years he was the acting manager of the Observatory at the Military School of Paris, and for thirty years afterwards he superintended that Observatory, as well as the publication of the *Connoissance des Temps*, the French Nautical Almanac. In that publication he gave, from time to time,

catalogues of the stars whose positions he had himself determined and which had not been observed before. The tenth publication of the stars thus observed carried their number to 11,300.

"When 70 years of age he published the first volume of his French Celestial History, which contains observations of 50,000 stars, made by himself and by other French astronomers. In his Astronomy, he has given a particular dissertation on achromatic telescopes; and his observations were made, as he himself informs us, with achromatic telescopes, of the finest construction by Dollond and Ramsden.

"The name of La Place carries its own sufficient eulogium; and the birth of Delambre, on the 19th of September, 1749, has been justly styled and recorded as being itself an epocha in the history of astronomy.¹²

"*Liverpool, 26th December, 1838.*"

"T."

¹² It is scarcely worth while to analyse Mr. Taylor's authorities, which, however respectable in themselves, and in their own times, prove nothing on the point in issue. Kepler only says, "*PERITI ARTIFICES negant ullam quantitatem, veluti rotundi corporis, detegi per inspectionem telescopii.*" He, therefore, speaks *second-hand* from what his *glass-grinders* told him. He was not and does not affect to be a great observer, but the contrary, for he says of himself, "*hebeti visu sum.*" He worked on Tycho's materials. The quotation from Horrocks shews that Spica is much less than Jupiter, and *not much greater* than one of Jupiter's satellites; which facts do by no means prove Mr. Taylor's case, seeing they go directly against it. They are conclusive against Lansberg, who gave the principal stars diameters of 30", and that is all. The following passage from Horrocks is that referred to by Lalande, § 2808,—"Galileus singulari modo observandi invenit diametrum stellæ fixæ primæ magnitudinis, non esse majorem 5 secundis, et forte nisi fixæ de proprio lucerent, multo adhuc minores apparerent; telescopium quo perfectius eo magis fixas representat ut mera puncta: quod etiam in appulsu Lunæ ad Pleyades patuit: ut primum enim Luna verum corpus fixarum texit, illico evanuerunt falsi radii, qui, si de vero corpore fuissent, sensim, et per gradus, nec omnes uno momento periissent."—*Hevelius, Mercurius in Sole visus*, p. 139. A classical passage.

Huyghens saw the stars as very small points when he used a smoked glass. It remains to be shewn how large his *small* points were (a second or two were not much thought of then), and whether the smoked glass took away nothing but *false* light. That the practice which Hevelius adopted of *limiting* the aperture *increased* the apparent magnitude, is worth noting.

Now (leaving Kepler's *on diu* out of the question), Horrocks and Huyghens lived before the invention of achromatic telescopes, and *for that reason* have no weight. If Mr. Taylor turns to Huyghens' *Dioptrica*, p. 116, or to Wood's *Optics*, p. 208, he will find that from the effect of chromatic aberration the stars could scarcely appear *points*, unless after all rays, except the brightest, are destroyed, either by admitting other light, or viewing them through a coloured glass. There are few greater astronomical names than Horrocks and Huyghens; but they are cited on a subject which never came under their cognisance, viz., the *appearances* of stars in powerful *achromatic* telescopes, with *high magnifying powers*.

If Gregory and Emerson are to be understood absolutely in Mr. Taylor's sense, they have spoken unguardedly, but this is pardonable, as they precede Sir W. Herschel. Mr. Taylor *contradicts* him.

Lalande (2808) says the apparent diameters of stars are extremely small,

Mr. Dawes replied to this somewhat miscellaneous list of authorities in the *Liverpool Times* of Jan. 8th, 1839. He draws attention to the fact, that all that he or Mr. Lassell had said

which is admitted; and, also, that we cannot determine their absolute size and true diameter, which is quite true, but not particularly applicable. In (2811), Lalande states a geometrical conclusion from certain hypotheses, which is as true as that two and two make four, and as much to the purpose.

I will add the following passage from Lalande, which is more to the point than Mr. Taylor's, see § 2810:—"Si l'on voit dans les lunettes une lumière éparsse qui environne les étoiles, qui les amplifie et les fait paroître comme si elles avoient 2' au même 6" de diamètre, on doit attribuer cette apparence à la vivacité de leur lumière, à l'air environnant et illuminé, à l'aberration des verres, à l'impression trop vive qui se fait sur la rétine. M. Herschel est parvenu dans un bon télescope à réduire le diamètre de la Lyre, à moins d'une seconde, et l'on parviendra peut-être à le diminuer encore plus."

Lalande, then, saw *apparent discs*, and *perhaps this is the reason why Mr. Taylor did not allude to this passage*. Except the impression on the retina, all the causes assigned by Lalande, who did not know the effect of *diffraction*, would leave the apparent diameter to be magnified by increasing the power. The reference to Herschel shews that some precautions were necessary to reduce the diameter of α Lyre to 1" in a reflector, which gives a smaller disc generally than a refractor. Suppose an apparent disc of 1".5 in a refractor, then with a power of 400, the star would subtend at the eye an angle of 10', a disc which will surely satisfy Mr. Taylor. Sir W. Herschel estimates the diameter of the larger star in Castor at 1".35 in a reflector of 7 feet focal length, 6.3 inches aperture, and power 460. *Phil. Trans.* 1803, p. 347, and the largest satellite of Jupiter at about 0".9.

The quotation from Laplace has been considered by Messrs. Dawes and Lassell, and I may say, without offence, that, in *practical astronomy*, Laplace speaks second-hand as well as Kepler. Those from Delambre, Nos. 29 and 30, are popularly true, but not as Mr. Taylor wishes them to be understood. That from Sir John Herschel is the oddest, as it gives a tolerably positive, though indirect, contradiction to Mr. Taylor's broad assertion, that "if a telescope in a clear sky make the disc of a fixed star discernible, it, as well as those who trust in it, had better be employed in other investigations than in those of astronomy." How is this to be reconciled with Sir John's statement, "The discs which good telescopes shew us of the stars are not real, but spurious?"

The rest of Mr. Taylor's letter is surplusage, and the praise of Lalande untrue. If Mr. Taylor really wishes to know and to speak accurately about Lalande, he should read the account of his life and analysis of his works, by his pupil, coadjutor, and friend, Delambre, *Histoire de l'Astronomie au XVIII^e siècle*. It tells us, p. 560, that "Lalande," wishing to observe the disappearance of Saturn's ring, "se transporta tout exprès à Béziers, sous le plus beau ciel de la France; mais comme il avait une vue très faible, son observation se trouva moins bon que celles des astronomes de Paris ou de Londres qui étaient restés chez eux." At page 565, *à propos* of the *Histoire Céleste*, Delambre says, "Ici Lalande ne paraît qu'éditeur. Il est vrai qu'il n'a réellement pris aucune part aux observations, et que de ces 50,000 étoiles, aucune n'a été déterminée par lui." So much for Mr. Taylor's "greatest observer the world has ever known." His real merits are stated a few lines further,—"S'il n'est à tous ces égards" (as a discoverer, observer, mathematician, and calculator), "qu'un astronome du second ordre, il a été le premier de tous comme professeur: plus

of the discs of fixed stars' was said of their *apparent* discs, as shewn in the *best* telescopes, which is the whole question; and that Sir J. Herschel, in T.'s quotation from him, speaks of the discs shewn by *good* telescopes. Mr. Dawes further says, that a power of from 30 to 40 for each inch of aperture is necessary to shew a disc, whereas Lalande generally observed, as was the custom then, with a lower power. He again refers to Sir W. Herschel, as quoted by Mr. Lassell, to shew that the discs of stars *are* increased by using higher powers, but *not* proportionally, and recommends persons who wish to possess a good telescope to choose that which shews a round apparent disc, rather than one in which "rays seem to stream like hair from a fixed star." In conclusion he remarks, that the parallax of a fixed star and the diameter of a fixed star are quite independent of each other.

Mr. Taylor's reply is in the *Liverpool Times* of Jan. 15, 1839:—

" ASTRONOMICAL ERRORS.

" To the Editors of the Liverpool Times.

" GENTLEMEN,—When your correspondent, the Rev. Mr. Dawes, spoke of a 'stellar disc,' he made no mention of spurious discs: the first mention of such a thing as a 'false disc' was made in my communication to the *Albion* of the 4th December. In his reply to that letter, Mr. Dawes gave several extracts from the writings of Sir John Herschel, in one of which the term 'spurious' is found; but to that word Mr. Dawes never alluded. In all their letters, Mr. Dawes and Mr. Wm. Lassell, jun. have both appeared perfectly confident in the reality of those discs, which I told them a month ago were false. Even so late as December 27, Mr. Wm. Lassell, jun. contended sturdily for the disc's reality. It is a pity that these gentlemen had not better understood each other, or made themselves better understood to plain readers; it would have saved the appeal to Kepler and his disciples.¹³

qu'aucun autre, il a su répandre l'instruction et le goût de la science." When Delambre adds, "*On lui reprocha des annonces futiles et trop fréquentes dans les journaux.*"—that "Lalande ne se faisait aucune scrupule de l'attaquer" (his old master, Lemonnier), "*avec cette vivacité imprudente qu'il mettait trop souvent dans la dispute.*"—that he charged Father Hell with falsifying his observations, because they were not first communicated to himself,—that he took every trifling opportunity to bring himself before the public; the failings of Lalande are well characterised; and these traits, which others will lament, may be the very qualities which Mr. Taylor, who has certainly no claim to any of Lalande's most valuable properties, has proposed to himself for imitation. Mr. Taylor cannot become either an astronomer, a mathematician, or bibliographer; but he may follow Lalande in one particular: he wrote, "*un discours sur La douceur, qu'il relisait tous les ans pour y prendre des règles de conduite auxquelles il a manqué trop souvent.*" I do not read that he was so silly or so hypocritical as to reproach other persons for plain speaking, but a special chapter may be added for his own use in Mr. Taylor's treatise.

¹³ Mr. Taylor must have had great confidence in the power of hardy

"When Mr. Dawes announced his having seen Encke's comet on the 27th September, I expressed my distrust in his observation, and gave as my reason, that the comet never had nor ever could have, the resemblance of a star of the tenth magnitude, whether seen in a haze or out of a haze, whilst the present physical constitution of the comet was preserved. To that reason fairly and distinctly alleged, Mr. Dawes has never made reply.¹⁴ I conclude, therefore, that he is by this time sensible that he was mistaken on the 27th September, in supposing that which he saw to be Encke's Comet.

"In conclusion, Mr. Dawes is wrong in supposing that I wish to take away the reputation of his telescope. That is not left for me to do, for there happens to have been an account of it published long ago, in the memoirs of the Royal Astronomical Society, vol. viii. page 63, by one whom Mr. Dawes highly venerates and loves, and who in return, I am assured, venerates and loves Mr. Dawes no less; and thus says he, of the workings of that identical telescope in his own skilful hands:—'From the discordances which exist among the measured distances by different observers, I have been led to pay great attention to this matter (the relative positions of double stars); not, however, with all the success I could desire. Many difficulties present themselves in this department. Not unfrequently, when the stars are well *defined*, they are found to vary in distance perpetually, and to an astonishing extent. At other times, though pretty steadily preserving their relative situations, they are moulded into such a variety of shapes, as to bid defiance to all attempts accurately to bisect them.'

"After such an account from an authority, which Mr. Dawes will confess to be quite equal to his own, we can hardly be asked to trust implicitly in the accuracy of that instrument.¹⁵

"*Liverpool, 11 January, 1839.*"

"T."

"P.S. The disc of the comet always covers a measurable space, even in its greatest elongation from us, which is not the case with fixed

assertion when he penned these sentences. His remarks, as well as those of Messrs. Dawes and Lassell, all along referred to stars *as seen in telescopes*, and to the assertion that a telescope which made the disc of a fixed star *discernible* was worthless. From the very nature of the dispute no other point could possibly be mooted.

¹⁴ How could Mr. Dawes reply to this silly remark, which Mr. Taylor calls a *reason*. He might as well call it "a mathematical truth." Mr. Dawes gave a description of what he saw, and this, however described, *must*, from its situation, have been Encke's Comet. Whether he described it *correctly* is a different question, and the only one which Mr. Taylor could properly debate.

¹⁵ This conclusion, from the preceding paragraph, is worthy of a distinguished place in the *memorabilia* of the Wrongheads. Mr. Dawes, in the passage cited by Mr. Taylor, sees and describes the effects of the atmosphere upon the appearance of double stars (every body who has looked through a telescope has seen the same thing), and Mr. Taylor lays the blame on the *instrument* and on the *observer*! I refer him to Sir W. Herschel. *Phil. Trans.* 1803, p. 217. "It is well known to astronomers that telescopes will act very differently at different times," &c.

stars even of the first magnitude. When removed to a distance the comet vanishes from our sight through the lessening of its light, which decreases in a proportion compounded of the proportion of the square of the comet's distance from the earth, and of the proportion of the square of its distance from the sun; whereas the apparent diameter decreases in the simple proportion of the distance of the comet from the earth alone."¹⁶

To this letter Mr. Dawes replied in the *Liverpool Times* of Jan. 22, 1839, in a letter to the Editors:—

"GENTLEMEN,—Were the communications of T. addressed to the Astronomical Society, though they reflect on myself and my instrument, it would be a work of supererogation in me to reply to them. In the eyes of such a body they would contain their own refutation. As it is, however, I am induced to trouble you with a few final remarks on the subjects adverted to in T.'s last letter. My time is too much occupied by more important matters to allow me further to prolong a correspondence which, from the eel-like qualities of my opponent, would seem likely to last out the year.

"If T. possesses a telescope capable of shewing the minute companion of Polaris, let him turn to that object, and imagine the light of a star of not more than half the brightness of that delicate point (which is of the ninth magnitude), diffused somewhat unequally over a space of some three or four minutes in diameter, and he will then possess a notion of the kind of object seen by me on September 27th. A star of the tenth magnitude was alluded to, as giving some idea of the quantity of light in the object altogether. Whether my description agrees with what T. would expect of Encke's Comet is a question about which I do not trouble myself; and I beg him to rest assured, that I was quite as well acquainted with its appearance and physical constitution before I ever heard of him or his productions as I am now. On what ground, therefore, he 'concludes' that I am by this time sensible that I was mistaken in supposing I saw it, it is not in my power to divine. In this conclusion he is as far in error as in many other topics of his communications. Indeed, it is amazing to me how he can dare to make assertions so palpably false as some of them are. For instance, in his last letter he declares that, 'even so late as December 27th, Mr. W. Lassell, jun., contended sturdily for the disc's reality.' Now, under date December 14th, Mr. Lassell says, 'Stars most certainly have *apparent* discs when seen through good telescopes; and those discs increase with the application of magnifying power, but *not by any means in an equal ratio*.' The last clause, alone, is sufficient to prove that Mr. L. did not need T.'s instructions on this point;

¹⁶ Mr. Taylor, who, if we may trust him, is quite at home when speaking of the "physical constitution of the comet," *assumes* that it continues of the same size during its course. I dare not contradict such authority, but surely the comet *may* change in volume (most vaporous and gaseous bodies *do*) as it recedes from, or advances towards, the sun. It is said to become less in its approach to the sun, which is odd enough if true.

and the use of the word 'apparent' in the beginning of the sentence would settle the question if any doubt remained. But is it possible that T. could really suppose that Mr. L. and myself were unacquainted with this commonplace fact, until *he* imparted to us of his profound stores of astronomical knowledge? With the evidence before him, surely he could not. In Vol. V. of the *Memoirs of the Astronomical Society*, page 145, will be found some remarks of mine on stellar discs, in reply to some queries addressed to me, in a private letter, from Sir John Herschel, respecting the comparative size of the apparent discs of fixed stars in my five-foot achromatic, and in reflectors. I there say, 'My conviction is, that the *apparent* discs are more affected by the *aperture* of the telescope than by any other circumstance; and that this is the principal reason why a reflector presents a smaller image of a fixed star than an achromatic of an equal illuminating power.' That is, the larger the aperture of the telescope the smaller the disc, *ceteris paribus*. I then proceed to state why I considered the discs shewn by my telescope to be small for the aperture.¹⁷ All this is distinctly at variance with the view of the subject which T., either ignorantly or disingenuously, labours to give.

"The only point I shall further notice is the quotation T. makes from my paper in the Astronomical Society's *Memoirs*, vol. viii. I have here to thank him for his running comment, which partakes of the same erroneous character as most of his assertions. The 'matter' to which I state I had paid great attention is not the '*relative positions* of double stars,' as T. expounds it, but the measures of their apparent *distances* from each other. I then refer to the difficulties attending the performance of these measures, arising from the unsteadiness and varying shapes of the stars, giving them the appearance of perpetually altering their distance and changing their form. It is really extremely difficult to give T. credit for such an amount of sheer ignorance on the

¹⁷ *Phil. Trans.* 1803, p. 345, Sir W. Herschel says, "From a number of observations and experiments I have made on the subject, it is certain that the diameter of a star in a reflecting telescope depends chiefly upon the four following circumstances: the aperture of the mirror with respect to its focal length, the distinctness of the mirror, the magnifying power, and the state of the atmosphere at the time of observation. By a contraction of the aperture we can increase the apparent diameter of a star so as to make it resemble a small planetary disc. . . . The effect of magnifying power is to occasion a relative increase of the vacancy between two stars that are very near each other; but the ratio of the increase of the distance is not proportional to that of the power, and, sooner or later, comes to a maximum." When good and careful observers like Huyghens, Herschel, and Dawes, remark any phenomenon, it is gratifying to see how this, in the progress of science, is generally found to be a *necessary* consequence of some *general* law. Thus Huyghens' remark on the increase of disc by diminished apertures, Herschel's statement that the discs were increased by increase of magnifying power, but not proportionally, and this conviction of Mr. Dawes, "that apparent discs are more affected by aperture than by any other cause," and "that the larger the aperture the smaller the disc," are all found to be *necessary* truths, since the investigation of Mr. Airy in the *Cam. Phil. Trans.* vol. v. p. 287, already cited.

subject he presumes to write upon, as that he should sincerely attribute these circumstances to the faultiness of the instrument employed. Yet, he is either thus ignorant, or his disingenuousness is rendered at once apparent. '*Utrum horum mavis, accipe.*' Hoping, however, that his malady is rather of the head than the heart, let me inform him that it is to the *bad state or qualities of the atmosphere* alone that the evils alluded to are attributable. Thus Sir J. Herschel, speaking of the atmospheric circumstances often attending the commencement of morning twilight, says (*Memoirs of the Astronomical Society*, vol. v. pages 48 and 49), 'As twilight comes on, they (the stars) begin to dance, and their discs appear to burst and fly to pieces, like drops of quicksilver let fall on a table, especially in eastern azimuth. In the west, at the same altitudes, their movements are less sudden, and they mould themselves into shapes with rounded angles, like large drops of mercury agitated but not broken. The phrases, 'twirling,' 'moulding,' 'convulsed,' 'twitching,' 'wrinkled,' 'burred,' 'glimmering,' used in the appended notes, are all expressive of peculiar movements and appearances of the discs of stars, and the rings which surround them; which have great influence on their measurement. The rarest of all states of the atmosphere is that in which the rings are destroyed, and the stars are seen perfectly round and tranquil.'—See notes on Nos. 333, 400, 487, and 488.

"It is refreshing to find something like correctness in the P.S to T.'s letter: but the truth it contains must be familiar to the veriest tyro in natural philosophy, inasmuch as it is applicable to all bodies shining by reflected light.

"Fully convinced, by the tenor of his correspondence, that my anonymous opponent is either too ignorant or too disingenuous to be worthy of further notice, I finally retire from the controversy.

"Yours respectfully,

"W. R. DAWES."

"Ormskirk, January 19, 1839."

Mr. Taylor replies thus in the *Liverpool Times*, Jan. 29, 1839:—

"To the Editors of the *Liverpool Times*."

"GENTLEMEN,—In your paper of this date the Rev. Mr. Dawes has favoured the public with a few 'more last words' on Encke's Comet, the Ormskirk Telescope, and on the letters to which the subjoined signature has been appended. In this, his last of all publications, Mr. Dawes has made use of certain terms as applicable to the simply expressing an unfavourable opinion of the accuracy of his astronomical observations, and of the intelligibility of his explanations of them: those phrases are 'eel-like qualities,' 'how he can dare to make assertions so palpably false,' 'his profound stores of astronomical knowledge,' 'ignorantly or disingenuously labours to give,' 'sheer ignorance on the subject he presumes to write upon,' 'or that he should

sincerely,' 'ignorant,' 'disingenuousness,' (and then comes a bit of Latin—save the mark!) 'malady rather of the head than the heart,' 'too ignorant or too disingenuous to be worthy of farther notice!'—A tolerable long string of abusive language this, to come out of the mouth of one whose weekly duty it is to preach patience, meekness, and moderation; and whose daily duty it is to practise these virtues. Let it pass. Mr. Dawes is evidently in a passion, and it is needless to reason with a man in that state. But the interests of science must not be sacrificed to any personal considerations; and I therefore shall reply to one or two paragraphs of his long letter, even at the peril of more abuse. When Mr. Dawes says that 'a star of the tenth magnitude was alluded to as giving some idea of the quantity of the light of the object altogether,' he gives a new version, and not an explanation of the old one. His original words were that 'its appearance was that of a hazy star of about the tenth magnitude,' an appearance which Encke's Comet never had, nor ever can have whilst it preserves its present physical constitution. It is therefore clear that Mr. Dawes did not see Encke's Comet on the 27th of September, 1838.¹⁸ When Mr. Wm. Lassell, jun., under date December 14th, said that 'the discs of stars increase with the magnifying power,' he shewed that he was not acquainted with that which takes place when good telescopes are used. The authorities from Kepler to Delambre, before given, will settle that point with any man of competent understanding, although they seem to be lost on Mr. Dawes.¹⁹ When Mr. Dawes affected 'to measure the distances of double stars from each other,' he affected to determine their relative position, which accordingly was the matter to which he professed to have paid great attention.²⁰ Praise in most instances is so pleasant (the student in *Don Quixote* was glad even of the praise of a mad man) that I might well be excused if I were to accept the commendation bestowed on the P.S. of my last letter; but it happens to be precisely that part of the subject

¹⁸ See notes 2 and 14.

¹⁹ See Mr. Lassell's authorities, and notes 12 and 17. When Mr. Taylor sets the authority of Sir W. Herschel at nought, Mr. Dawes, Mr. Lassell, and I may be content with his disregard of ours.

²⁰ Mr. Taylor must learn to quote *verbatim*, and not to paraphrase. In double star measures, *position* is a specific term; the *angle of position* is the angle which the line joining the star and its companion makes with the parallel of daily motion, or, in Sir J. Herschel's language, with the meridian. The *distance* is the number of seconds of space in this joining arc. Now it is the last kind of measure which, without clock-work, presents the greatest difficulty, and is most affected by a disturbed atmosphere, and it is to measures of *distance* which Mr. Dawes had paid great attention. If Mr. Taylor were really ignorant of the European reputation of the Ormskirk observer, as one of the nicest, if not the very nicest measurer of delicate objects now living, it shews that he is very much out of the way of current astronomical information, and not quite the person to lead the scientific public of Liverpool. If Mr. Taylor knew better, there is another way of accounting for his conduct; but I stand up for his ignorance, partly on charitable grounds, but principally from believing that no man dare make such an exhibition of himself ~~unwittingly~~ and knowingly."

of which Mr. Dawes is the least capable of judging. I must forego it, and leave him to inflict his praise on some one more unlucky than I happen to be.

"T."

"*Liverpool, January 22, 1839.*"

On a review of the whole controversy, it is evident that Mr. Taylor is wrong in every position. It is clear that Mr. Dawes saw the comet on Sept. 27th, and described it correctly; it is certain that a good telescope, in a clear sky, shews the stars with discs, which are increased by increasing the magnifying power; and it will scarcely be doubted, that, in imputing the ordinary effects of a disturbed atmosphere to faultiness in the instrument or the observer, Mr. Taylor has given a remarkable instance of his talent for misapprehension, "wilful or not wilful."

In turning over the London *Times* to examine the precise notices respecting Encke's Comet, Breslaw, &c., I hit upon a letter of Mr. Taylor's, dated Nov. 13th, and inserted on November 16th, 1838. He first mentions Gautier's idea that the errors of Encke's *Ephemeris* may probably arise from the perturbations caused by Mercury, and then proceeds as follows:—

"It is quite as likely that the error may have arisen from M. Encke having gone on his old, but very questionable, notion of the existence of a resisting medium pervading the sphere of the sun's attraction."²¹ "On Tuesday last, at forty-three minutes past six, the comet was seen here in a right line between and equidistant from the two stars α and ρ of the constellation of the Dragon (Nos. 2234 and 2243 of the *Catalogue of the Astronomical Society*), which would give a right ascension of $19^h 10^m$, and a north declination of $57^\circ 1' 28''$, for the comet's place at that time. This would make the *Ephemeris* in error at that instant $1^m 22^s$ in time for right ascension; and more than $9' 36''$ for north declination, which would together amount fully to twelve minutes in space.²² It would, then,

²¹ Mr. Taylor knows as much of the effects of a resisting medium as he does of the "method of least squares," or of the "photographic delineation of nebulae;" but that is no obstacle to his exercising his "right to form and make public his opinions on points of physics and astronomy." I also assume my right to designate such *impertinence* by its proper name, and others may judge which is the better right. The opinion of the Astronomer Royal was so strongly in favour of the hypothesis of a resisting medium that he translated, published at his own expense, and distributed, Encke's memoir.

²² So far as I know, this is Mr. Taylor's *single* observation, and I have had the curiosity to examine it. *Ex pede Herculem*; though a sample be small, it may still yield satisfactory information as to the nature of the commodity:—

"*On Tuesday last.*" Tuesday last, if the *Nautical Almanac* is to be

appear that M. Encke has calculated that the comet would be nearer

trusted, was, November 6th, a date which will by no means suit the observations. For *Tuesday* read *Monday*, November 5, 1838.

"At forty-three minutes past six." "Under which clock, Romanian?" Astronomers generally give some information on this point, Mr. Taylor does not, so we must compute on the double hypothesis of Greenwich or Liverpool time. The comet's apparent place from the *Ephemeris* is, if Mr. Taylor used—

	Right Ascens.	North Declin.
Greenwich time.....	19 ^h 8 ^m 39 ^s ·5	57° 28' 38"
If Liverpool time	19 8 28·0	57 27 17

The stars mentioned are best known by the names 53 and 54 Draconis. Bringing them up to November 5th, I find their apparent places—

	Mag.	Right Ascens.	North Declin.
53 Draconis	5	19 ^h 8 ^m 36 ^s ·0	56° 35' 21"
54	5	19 11 0·9	57 25 53
Mean		19 9 48·5	57 0 37

Now as the comet was "exactly in a right line and equidistant from the stars," this must be pretty nearly the place of the comet, neglecting parallax and refraction; and my result agrees pretty well with Mr. Taylor's. Hence, Mr. Taylor's errors of the *Ephemeris*, according as he used Greenwich or Liverpool time, are,—

	Right Ascens.	North Declin.
Greenwich time.....	— 1 ^m 9 ^s ·0	+ 28' 1"
Liverpool time	— 1 20·5	+ 26 40

Now Mr. Dawes gives (see *Liverpool Times*, Dec. 4) for the errors of the *Ephemeris* on Nov. 5—

Right Ascens. — 1 ^m 39 ^s ·1	North Declin. + 3' 41"
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which are so different from Mr. Taylor's, especially in declination, that both cannot be right. Mr. Dawes is not in the wrong, because he can observe a comet correctly, as has been shewn above; because he agrees with Greenwich on the 9th, the only corresponding day; and because his error on the 5th harmonises not only with his own results but with those of Greenwich and Cambridge. I mention these corroborations of Mr. Dawes, not that he requires any such testimony, but to take from Mr. Taylor a plea for quibbling.

A mistake of 24' in dividing a total distance of 54' (for that is the distance between 53 and 54 Draconis) into two equal parts is impossible. Let any one map the configuration, and he will say at once that neither Mr. Taylor nor any one else could have seen what he states, viz. the comet "in a right line between and equidistant from" 53 and 54 Draconis. The fabrication of an observation without any conceivable purpose is something as monstrous as to be incredible. To clear the matter up, I laid down the stars from the Catalogue, the comet from Mr. Dawes's place, and prolonged the line from 54 Draconis through the comet, when I found that a star with a right ascension of about 19^h 9^m and north declination 57° 23' would, if it existed, satisfy Mr. Taylor's observation. The same is not in the *Histoire Céleste*, nor in any catalogue I have now at hand, so I looked for it in the sky, and sure enough thereabouts I found a star of the seventh magnitude, which precedes 54 Draconis 1^m 48^s·1, with a declination about 3' less than that star. Supposing Mr. Taylor to have seen this star, and mistaken it for 53

to the sun by 500,000 miles, and nearer to the earth by 30,000 miles, when they passed each other than it has actually been."²³

There is some description of the appearance of the comet, &c. which I don't understand, but it is very grandly and figuratively written. The mounting of the telescope is said to be rude and the results only approximative.²⁴

"Such may, nevertheless, be serviceable in the absence of those other and better observations which the public has a right to have

Draconis, his *observed* place of the comet will be the same as that of 54 Draconis after *half* these differences are respectively subtracted. Hence we have—

	Right Ascens.			North Declin.			
Observed place of Comet	= 19 ^h	16 ^m	6 ^s ·9	= + 57°	24'	23"	Mr. Taylor corrected.
Tabular place (Li- verpool time)	19	8	28·0	+ 57	27	17	<i>Ephemeris</i> .
Errors of <i>Ephemeris</i>	-1	36·9		+	2	54	Mr. Taylor corrected.
Errors of <i>Ephemeris</i>	-1	39·1		+	2	41	{ Mr. Dawes (<i>Liverpool Times</i> , Dec. 4).

This close agreement is the result of chance; there is, doubtless, some compensation of error. I have computed the comet's place *accurately* from the *Ephemeris* (Mr. Dawes probably used the hourly motion there given), and the places of the stars from the Oxford catalogue: the declination of the new star may be wrong 30" or more, as I only measured the difference by the setting circle of my transit. I am glad to have been able thus to clear up what seemed a grave suspicion with respect to the truth of Mr. Taylor's observation, yet in this very simple affair how many mistakes has he committed!—A *wrong* day, an *uncertain* clock-time, a *wrong* star of comparison! Without the guidance of Mr. Dawes, it would have been a hopeless task to attempt setting this hodge-podge right, and a dozen good observations might have been made in less time than it has cost me. How Mr. Taylor got *his* place of the comet to differ only 9' 36" from the *computed* place, *instead of 26' or 27' which his data shew*, he must explain, I cannot; and I suggest to him that this is absolutely necessary, otherwise we must be compelled to believe that he *could not*, at least *did not*, compute the tabular place of the comet, *though he professed so to do*, but *assumed* the error in declination to support some hypothesis of an error of 12', or that he *appropriated* it ("convey, the wise it call") from foreign accounts, which correspond to an earlier date. The error of 9' or 10' is, in fact, the error of the *Ephemeris* in declination a week or ten days previous to Nov. 5th, and Mr. Taylor's letter is dated November 13th. I wish him "a good deliverance," for I cannot help him in this dilemma.

I may mention here that it is customary in stating errors to give the *direction*, i. e. whether the *Ephemeris* errs in *excess* or *defect*; or, what is better still, to give the *correction* of the *Ephemeris* with the proper algebraic sign to be applied to the data of the *Ephemeris*.

²³ I do not see the links which connect Mr. Taylor's *presumed* facts with his conclusions. It is always desirable to have a clue by which to trace the results which are so nakedly stated, and Mr. Taylor requires an Ariadne's aid as much as his hero Theseus.

²⁴ A person who thinks himself called upon to "make public his opinions" might have purchased a circular micrometer, or some sort of reticule, which, in good hands, would have supplied any defects in the mounting. But a bad workman has always bad tools.

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trusted, was November 6th, a date which will by no means suit the observations. For *Tuesday* read *Monday*, November 5, 1838.

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trusted, was November 6th, a date which will by no means suit the observations. For *Tuesday* read *Monday*, November 5, 1838.

"At forty-three minutes past six." "Under which clock, Romanian?" Astronomers generally give some information on this point, Mr. Taylor does not, so we must compute on the double hypothesis of Greenwich or Liverpool time. The comet's apparent place from the *Ephemeris* is, if Mr. Taylor used—

	Right Ascens.	North Declin.
Greenwich time.....	19 ^h 8 ^m 39 ^s ·5	57° 28' 38"
If Liverpool time	19 8 28·0	57 27 17

The stars mentioned are best known by the names 53 and 54 Draconis. Bringing them up to November 5th, I find their apparent places—

	Mag.	Right Ascens.	North Declin.
53 Draconis	5	19 ^h 8 ^m 36·0	56° 35' 21"
54	5	19 11 0·9	57 25 53
Mean		19 9 48·5	57 0 37

Now as the comet was "exactly in a right line and equidistant from the stars," this must be pretty nearly the place of the comet, neglecting parallax and refraction; and my result agrees pretty well with Mr. Taylor's. Hence, Mr. Taylor's errors of the *Ephemeris*, according as he used Greenwich or Liverpool time, are,—

	Right Ascens.	North Declin.
Greenwich time.....	— 1 ^m 9 ^s ·0	+ 28' 1"
Liverpool time	— 1 20·5	+ 26 40

Now Mr. Dawes gives (see *Liverpool Times*, Dec. 4) for the errors of the *Ephemeris* on Nov. 5—

Right Ascen.	— 1 ^m 39 ^s ·1	North Declin.	+ 3' 41"
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which are so different from Mr. Taylor's, especially in declination, that both cannot be right. Mr. Dawes is not in the wrong, because he can observe a comet correctly, as has been shewn above; because he agrees with Greenwich on the 9th, the only corresponding day; and because his error on the 5th harmonises not only with his own results but with those of Greenwich and Cambridge. I mention these corroborations of Mr. Dawes, not that he requires any such testimony, but to take from Mr. Taylor a plea for quibbling.

A mistake of 24' in dividing a total distance of 54' (for that is the distance between 53 and 54 Draconis) into two equal parts is *impossible*. Let any one map the configuration, and he will say at once that neither Mr. Taylor nor any one else could have seen what he states, viz. the comet "in a right line between and equidistant from" 53 and 54 Draconis. The fabrication of an observation without any conceivable purpose is something as monstrous as to be incredible. To clear the matter up, I laid down the stars from the Catalogue, the comet from Mr. Dawes' place, and prolonged the line from 54 Draconis through the comet, when I found that a star with a right ascension of about 19^h 9^m and north declination 57° 23' would, if it existed, satisfy Mr. Taylor's observation. The zone is not in the *Histoire Céleste*, nor in any catalogue I have now at hand, so I looked for it in the sky, and sure enough thereabouts I found a star of the seventh magnitude, which precedes 54 Draconis 1^m 48^s·1, with a declination about 3' less than that star. Supposing Mr. Taylor to have seen this star, and mistaken it for 53

to the sun by 500,000 miles, and nearer to the earth by 30,000 miles, when they passed each other than it has actually been."²³

There is some description of the appearance of the comet, &c. which I don't understand, but it is very grandly and figuratively written. The mounting of the telescope is said to be rude and the results only approximative.²⁴

"Such may, nevertheless, be serviceable in the absence of those other and better observations which the public has a right to have

Draconis, his *observed* place of the comet will be the same as that of 54 Draconis after *half* these differences are respectively subtracted. Hence we have—

	Right Ascens.	North Declin.	
Observed place of Comet.....	= 19 ^h 16 ^m 6 ^s .9	= + 57° 24' 23"	Mr. Taylor corrected.
Tabular place (Li- verpool time).....	19 8 28.0	+ 57 27 17	Ephemeris.
Errors of Ephemeris	-1 38.9	+ 2 54	Mr. Taylor corrected.
Errors of Ephemeris	-1 39.1	+ 2 41	{ Mr. Dawes (Liverpool Times, Dec. 4).

This close agreement is the result of chance; there is, doubtless, some compensation of error. I have computed the comet's place *accurately* from the *Ephemeris* (Mr. Dawes probably used the hourly motion there given), and the places of the stars from the Oxford catalogue: the declination of the new star may be wrong 30" or more, as I only measured the difference by the setting circle of my transit. I am glad to have been able thus to clear up what seemed a grave suspicion with respect to the truth of Mr. Taylor's observation, yet in this very simple affair how many mistakes has he committed!—A *wrong* day, an *uncertain* clock-time, a *wrong* star of comparison! Without the guidance of Mr. Dawes, it would have been a hopeless task to attempt setting this hodge-podge right, and a dozen good observations might have been made in less time than it has cost me. How Mr. Taylor got *his* place of the comet to differ only 9' 38" from the *computed* place, *instead of 26' or 27' which his data shew*, he must explain, I cannot; and I suggest to him that this is absolutely necessary, otherwise we must be compelled to believe that he *could not*, at least *did not*, compute the tabular place of the comet, *though he professed so to do*, but *assumed* the error in declination to support some hypothesis of an error of 12', or that he *appropriated* it ("convey, the wise it call") from foreign accounts, which correspond to an earlier date. The error of 9' or 10' is, in fact, the error of the *Ephemeris* in declination a week or ten days previous to Nov. 5th, and Mr. Taylor's letter is dated November 13th. I wish him "a good deliverance," for I cannot help him in this dilemma.

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from those who are so well paid for making them; or rather, as they would seem to think, for not making them."²⁵

²⁵ I will add a few words on this finishing sentence, in which the temper and knowledge of our newspaper *scavengers* are fairly exhibited. The public pay for two observatories—Greenwich and Edinburgh; and one observer, Sir James South, has an allowance of 300*l.* a-year *to aid him in his astronomical researches*. Now Greenwich is under the control of the Admiralty, advised by a board of visitors, and I am certain that the Astronomer Royal has received no directions to write in the newspapers: so he is blameless. Edinburgh, in like manner, is responsible to the Home Secretary; but I should be surprised if, in the instructions given to the director, any allusion was made to the daily press as a vehicle of communication. The directors of these establishments give their observations to the world yearly in a substantial form, and with a detail and accuracy which leave all foreign publications far behind. As to the observer of Campden Hill, the public must be very unreasonable if it is not satiated with the lengthy epistles which appear from him in the *Times*. Some persons, indeed, think that if he would observe a little more and scribble a little less, or more to the purpose, he would better justify Sir Robert Peel's munificence than he has done hitherto. Fifteen pensioned years have passed without one sterling observation making its appearance from that beautiful observatory, so far as I know.

"2 *Serv.*—These fifteen years you have been in a dream;
Or, when you waked, so waked as if you slept.

S.—These fifteen years! By my fay, a goodly nap.
But did I never speak of all that time?

1 *Serv.*—Oh, yes! my lord; but very idle words."

Perhaps Mr. Taylor meant Sir J. South; at any rate, *he* is the only observer on whom, *as such*, the public have a *direct* claim. The Lords of the Admiralty, the Greenwich visitors, and the Home Secretary, are the proper objects of *mobbing* for the omissions of the national observatories, when Mr. Taylor is in the mood for enlightening the world on "points of physics and astronomy."

When a comet is discovered, when the elements of the orbit are determined, or when an ephemeris is computed, a newspaper does real service by spreading the information more widely and rapidly than any private person or association can do, and accordingly all discoveries and computations of this kind are communicated by the daily press. Take as specimens Schumacher's Circulars, or Mr. Hind's Notices. Unfortunately, some contributors make a comet the pretext for venting bad temper, party feeling, or knavish insinuation. Others think themselves qualified to give "opinions on physics and astronomy," and inclose their stolen jewels in a setting of their own crude and twaddling speculation, imposing on the credulity of the unlucky reader, and taking credit for their spoiled plunder. An editor might surely decipher such charlatans without being a conjurer.

No. III.

PHOTOGRAPHY AT ROME.

HITHERTO Mr. Taylor has been viewed as an astronomer, a severe critic on observatories and observations, on the use and abuse of instruments; at one moment disapproving of the conduct of the *Nautical Almanac*, sparring now at Encke, now at Boguslawski, and "proving quite satisfactorily to any man of understanding" that the Herschels and South, Dawes and Lassell, could not have seen, what they profess to have seen, in their telescopes. He is at present to be considered as a man of letters, exercising the useful task of introducing the labours and discoveries of scientific foreigners to the English public.

I reprint, as a specimen of his talents and his successful application of them in this honourable direction, the following communication to the *Liverpool Times* of July 30th, 1842:—

"Astronomy and Photography at Rome.

"A Memorial of sundry Observations made at the Observatory of the Gregorian University, in the Collegio Romano, by the Director, P. Francisco de Vico, and the other Astronomers of the Company of Jesus, in the Years 1840 and 1841. Rome, 1842: Marini and Co. Printers.

"This publication, of which a copy has been, with the greatest courtesy, transmitted to this town, will command the attention of the scientific world, not only by the important information contained in it, but also as being the precursor¹ of a series of annual memoirs, intended to contain reports of future astronomical operations at Rome, in the observatory of the Collegio Romano, which will henceforth take its place in the first rank of that class of the European scientific establishments.²

"The observatory at the Collegio Romano may, in fact, be considered as the oldest in Europe, having been the station from which Clavius made his observations on the new star of the constellation of Cassiopeia, in the year 1572. From that time it had in succession for its superintendents, the Jesuit Scheiner and the illustrious Cassini, followed by Bianchini and Boscovich, who died in 1787.³

¹ This precursor of a series is the *third*, and the preceding numbers are referred to in the very memoir which Mr. Taylor calls the *precursor*.

² A flourish intended to repay Father De Vico for a presentation copy. The memoir itself lays no claim to the rank which Mr. Taylor, *mero motu et speciali gratiâ*, grants to it, being a plain, unpretending performance.

³ De Vico says, note, page 5, "Può dirsi che il Collegio Romano sia stato un osservatorio astronomico continuato da Clavio in poi." But he goes on

"The wars of the French Revolution interrupted for thirty years the peaceful pursuit of astronomy at Rome; but, on the restoration of peace, in the year 1816, Pope Pius VII. constructed the present observatory, which Leo XII. in 1824, restored, along with the rest of the Collegio Romano, to the order of the Jesuits. Since that time the observatory has enjoyed the particular patronage of the generals of the order, the set of instruments having been augmented by a famous refracting telescope of Cauchoix, by an astronomical theodolite of Gambey, by an excellent chronometer of Breguet, and a capital meridian circle of Ertel.⁴

"Whether it be through the superior excellence of this particular telescope, or the greater clearness of the Roman sky, the services obtained from it have been most efficient, and such as may give a new starting point for the science of astronomy.⁵

"By the reports given in the *Memorial* just published, the advantages are fully shewn which may be derived from observations of the periodical falling stars, in corroboration of lunar and planetary observations, for the accurate determination of the relative position of places not otherwise attainable by geodesical measurement. The success of the operations used in the instances of Rome, Naples, and Palermo, as respects each other, fully establishes the fact. The corrections obtained by these means have been adopted by the French astronomers in the *Connaissance des Temps*, although our *Nautical Almanac* is still in error. Taking the difference in longitude of Greenwich and of Paris at 9 minutes 21.5 seconds, the correct position of the observatory at Naples will be $0^h 57^m 1^s.5$, longitude east of Greenwich, and $40^\circ 51' 46'' 6$ north latitude; lessening by $7^s.8$ the longitude hitherto given for Naples in the *Nautical Almanac*. The longitude of the Collegio Romano, at Rome, will then be $0^h 49^m 55^s.27$ east of Greenwich, and its latitude $41^\circ 53' 52''$ north, increasing by $0^s.57$, the longitude hitherto given in the *Nautical Almanac*. It is to be observed that there is a difference of half a second of time in the longitude of Paris and Greenwich, between the reckoning of the *Connaissance des Temps* and that of the *Nautical Almanac*, which

to say that Clavius' observations were probably made *nel Collegio vecchio*; and from the context it may, I think, be inferred that the "station" was not the same. Certainly he speaks less decisively than Mr. Taylor; and I have no map which shews the relation of the *Specula Gregoriana* to the Jesuits' Church, which would settle the matter. Father De Vico mentions the observers who followed Clavius, but he does not name the "illustrious Cassini" or "Bianchini" in the number. Did De Vico forget these ornaments of the Collegio Romano, or did Mr. Taylor add them of his own knowledge; and if so, where did he get it?

⁴ "Lo accresce ora il secondo (*i.e.* general of the order) con un eccellente e nuovo Circolo-Meridiano di Ertel." If I mistake not, this differs from Mr. Taylor's statement, "this set of instruments having been augmented by a capital meridian-circle of Ertel." The instrument was ordered, but not received when De Vico wrote; and therefore no part of the existing "set of instruments." Tenses are of importance in translating.

⁵ Will Mr. Taylor explain specifically what this new starting point is? I can discover none such in the memoir.

might as well be reconciled, seeing that they are both such great authorities, and Paris and Greenwich such noted astronomical stations.⁶

By corresponding observations of falling stars, at the two observatories of the *Specula Gregoriana* in the Collegio Romano at Rome, and of *Capo di Monti*, the observatory of Naples, the difference in longitude was found to be,—

	^h	^m	^s
In 1839	0	7	5.70.
1840	0	7	5.77
Mean	0	7	5.74
From transits of the moon in 1828		^m	^s
1829		7	6.48
From lunar occultations in 1829-1830		7	5.97
		7	5.90

De Vico takes $7^m 5.76$ as the mean.

Now I remark, with regard to Mr. Taylor's comments, that the *fullness* he speaks of is nowhere to be found in this Memoir, which states *results* only;—(some particulars, from which an estimate of accuracy can really be made, are given in the memoir for 1839, p. 28, which Mr. Taylor had never seen or heard of;—that the word “corroboration” is incorrect, and the word “planetary” sheer nonsense. Finally, that “geodesical measurement,” when “attainable,” does not necessarily give a true astronomical difference of longitude, unless a correct figure of the earth be assumed, and unless there is a certainty of the non-existence of local disturbing attraction.

I am not quite satisfied with the manner in which Daussey and De Vico have combined their data in calculating the longitudes of Rome and Naples. The Roman astronomer has modestly derived Rome from Naples, the French hydrographer has, with more propriety, made Rome tell on Naples. The excellent triangulation of Fergola is introduced by Daussey to connect Milan, Rome, Naples, and Palermo; but it seems to me that geodesical values are not admissible when discussing *primary* points at such a distance from each other. I would proceed thus:—

(1.) Longitude of Naples, *Capo di Monti*.

The difference between Palermo and Naples,	^m	^s
by falling stars	3	36.3 <i>Nobile</i> .
by chronometers	3	34.7 <i>Daussey</i> .
Mean	3	35.5
Longitude of Palermo	44	4.0 <i>Daussey</i> .
Difference of Palermo and Naples	3	35.5
Derived Longitude of Naples	47	39.5
Independent Longitude: 19 eclipses of ☉		
and occultations, Wurm and others ...	47	42.0 <i>De Vico</i> .
Longitude of Naples	47	40.75

(2.) Longitude of Rome, *Collegio Romano*.

Longitude of St. Peter's, by 6 eclipses ☉		
and 5 occultations	40	26.71 <i>Daussey</i> .
Observatory east of St. Peter's		6.54 <i>Fergola</i> .
Independent Longitude	40	33.25

Now, the difference between Rome and Naples, by falling stars in the years 1839, 1840, is $7^m 5.74$ (*De Vico*), which seems from other observa-

The mode of determining the longitude by observations of falling stars was first suggested by Dr. Maskelyne, in the year 1783, and was made

tions to be a little in defect; take $7^m 5^{\cdot}8$ for the true difference. This result should be very accurate, and is $1^{\cdot}7$ less than that which is obtained from the unconnected longitudes. The repartition must be somewhat arbitrary as the partial results of each longitude are not before us. Giving a slight preference to Naples as resting on more observations, and as connected with Palermo, I subtract $0^{\cdot}75$ from Naples and add $0^{\cdot}95$ to Rome, when the final longitudes *East of Paris* are,—

		Daussey.	De Vico.
Rome, <i>Collegio Romano</i>	$40^m 34^{\cdot}2$	$40^m 33^{\cdot}9$	$40^m 33^{\cdot}8$
Naples, <i>Capo di Monti</i>	$47^m 40^{\cdot}0$	$47^m 39^{\cdot}8$	$47^m 40^{\cdot}5$

To compare these results with the *Nautical Almanac*, add $9^m 21^{\cdot}5$ to each, and we have,—

		Daussey.	De Vico.	Nautical Almanac.
Rome	$49^m 55^{\cdot}7$	$49^m 55^{\cdot}4$	$49^m 55^{\cdot}3$	$49^m 54^{\cdot}7$
Naples	$57^m 1^{\cdot}5$	$57^m 1^{\cdot}3$	$57^m 2^{\cdot}0$	$57^m 0^{\cdot}3$

It would be idle to give a decided preference to any of these results; they are all within the limits of reasonable error; but it seems to me that *in principle* the way I have followed is the surest. Where Mr. Taylor found his error of $7^{\cdot}8$ in the *Nautical Almanac* I do not know. The longitude given here has been inserted in the *Nautical Almanac* since 1835.

The superintendent of the *Nautical Almanac* states the longitudes of the observatories as they are communicated to him by the directors of those establishments, or, in defect of such information, from the best published accounts; but it is not desirable that they should be frequently altered, or that the superintendent should take upon himself to decide upon imperfect data. M. Daussey is the *hydrographer* of France, and setting maps and charts right is his business. Mr. Taylor is for once correct in one opinion; it *is* to be wished that the difference of longitude between Greenwich and Paris should be determined with the greatest accuracy, seeing that they are “such noted astronomical stations;” and when the railroad is brought from Paris to Boulogne or Calais, no doubt it will be so determined *chronometrically*, just as the arc from Greenwich to Valentia, including Liverpool, was determined last summer. If there were an observatory at Havre, like the Liverpool Observatory, I would have promised it earlier. Mr. Taylor is not aware of the precise nature of the operation, or where the difficulty lies. The difference of $0^{\cdot}5$ “between the reckoning of the *Connaissance des Temps* and the *Nautical Almanac*,” which now startles him, is a difficulty to him alone. The *Connaissance des Temps* gives the longitude in time to a whole number of seconds only. If Mr. Taylor had used his eyes, he might have seen this, and also that the longitude in arc is given to seconds. Now, reducing $2^{\circ} 20' 24''$, the longitude in arc, into time, the longitude of Paris comes out $9^m 21^{\cdot}6$ east of Greenwich; thus differing $0^{\cdot}1$ from the *Nautical Almanac*. In whole numbers, $9^m 21^{\cdot}6$ become $9^m 22^{\cdot}$.

Perhaps Mr. Taylor is not aware of Sir J. F. W. Herschel's memoir upon the longitude of Paris, in the *Philosophical Transactions*, 1826, p. 77, &c. Sir John says, p. 126, “On the whole, then, $9^m 21^{\cdot}6$ may be assumed as a result not very likely to be altered a whole tenth of a second.” Possibly Mr. Taylor may now feel satisfied that the “difference between these great authorities” is not so irreconcilable as he imagined, and be obliged to me for teaching him how to *spell* the *Connaissance* for the future.

use of in 1802, in Germany; but it was reserved for the Neapolitan astronomer, Nobile, to perfect the method, and to point out the right way to be followed in the practice of those observations.⁷

“*Photographical Delineation of Nebulæ.*”

“One of the first uses made by Galileo of the telescope, on its invention, was the examination of some of the most remarkable nebulæ, and the delineation of their then state, as if he had anticipated future changes in their constitution. In his *Siderius Nuncius*, published in the year 1610, he gave drawings, upon a large scale, of the Pleiades, of the Belt and Sword of Orion, of the nebula in the head of Orion, and of the cluster of stars known as the Præsepe, or bee-hive, in the constellation Cancer. In these, the ground is black and the stars white. The positions of the stars are given with considerable precision; but there is no trace of the remarkable extent of lucid nebulous matter, nor of the deep black indenture and distinct outline which gives it something of the appearance of a bat's wing. The idea of perpetuating the appearance of this particular nebula in this time was taken up by Huyghens in the year 1656; and he has left what he vouches for a correct representation of it, as seen by him, but unaccountably passes over in silence the drawing left by Galileo.⁸ In

⁷ I have looked at Nobile's directions, which do not seem to me to contain any peculiar novelty, or any thing not known before. The coincidence of the partial results in the specimen given in De Vico's Memoir for 1839, p. 28, is very close; but it is for quickness of observation and perseverance that praise is due, and not for discovery.

⁸ The mention of Galileo, in reference to the great nebula of Orion, is a blunder, or else most wanton surplusage. He delineated, indeed, in a rude way, the stars of the Belt and Sword of Orion; but there is not a syllable of the *nebula*, which he most probably never saw—certainly never mentioned. After explaining the cause of the appearance of the milky way, viz. *nihil est aliud quam innumerarum Stellarum coacervatim consitarum congeries*, he adds, “At cum non tantum in GALAXIA lacteus ille candor, veluti albicantis nubis spectetur, sed complures consimilis coloris areolæ sparsim per æthera subfulgeant, si in illarum *quamlibet* Specillum convertas, Stellarum constipatarum cœtum offendet. Amplius (*quod magis miraberis*) Stellæ ab Astronomis singulis in hanc usque diem NEBULOSÆ appellatæ, Stellarum mirum in modum consitarum greges sunt, ex quarum radiorum commixtione, dum unaquæque ob exilitatem, seu maximam à nobis remotionem, oculorum aciem fugit, candor ille consurgit, qui densior pars cœli, Stellarum, aut Solis radios retorquere valens, hucusque creditus est. Nos ex illis nonnullas observavimus, et duarum Asterismos subnectere volumus.” These two are the nebulous star of the *head* of Orion and the nebulous star of Præsepe, each of which is shewn to be a congeries of stars, too close to be separately discerned with the naked eye. Perhaps Mr. Taylor has confounded the ancient “*nebulosa*” in the head with the new “*nebula*” in the Sword-belt of Orion; but Galileo had clearly no notion of *irresolvable nebula*.

Huyghens' telescope had 23 feet focal length, with an aperture of 2½ inches, and a compound eye-piece, equivalent to a single lens of 3 inches focus or rather less. The magnifying power was, therefore, about 100. Galileo used a much inferior instrument with a power of 30. Huyghens relates his discovery of the great nebula of Orion in the following words:—

“Unum verò circa fixas phænomenon relatu dignum occurrit, à nemine

Huyghens' drawing and description, the shape of the nebula differs considerably from that which it now has, and the engraving in Sir John F. W. Herschel's *Astronomy* for the year 1833, is still more at variance with the present reality.⁹ In these circumstances a doubt

hucusque, quod sciam, animadvertum, nec quidem nisi grandibus hisce telescopiis recte observandum. In Orionis ense tres stellæ ab Astronomis reponuntur inter se proximæ. Harum mediam Anno 1656 fortè per tubum insipienti mihi, pro stellâ unâ duodecim (quod quidem minime novum) sese obtulerunt; eo posito quem subjectâ figurâ expressimus.

"Ex his autem tres illæ pene inter se contigunt, eunque his aliæ quatuor, velut trans nebulam hæbant, ita ut spatium circa ipsas, quæ formâ hic conspicitur, multo illustrius appareret reliquæ omni celo; quod cum apprime serenum esset et cerneretur nigerrimum, velut hiatu quodam interruptum videbatur, per quem in plagam magis lucidam esset prospectus. Idem verò in hanc usque diem nihil immutata facie sæpius atque eodem loco conspexi; adeo ut perpetuam illic sedem habere credibile sit hoc quiddam est portentum; cui certè simile aliud nusquam apud reliquas fixas potui animadvertere. Nam cæteræ nebulosæ olim existimatae, atque ipsa via lactis, perespicio inspectæ, nullas nebulas habere comperiuntur, neque aliud esse quam plurium stellarum congeries et frequentia." — *Christ. Hugenii Opera*, Lugd. Bat., 1724, p. 540.

This extract shews that Galileo could not, in Huyghens' opinion, have seen the nebula for want of optical means; but he was not given to stuff irrelevant matter into his memoirs. It is odd that Huyghens never looked at the nebula of Andromeda, if he had heard of Simon Marius's discovery. In this, as in most other instances, Mr. Taylor is unlucky, and contrives to get hold of any thing but the handle.

Le Gentil remarks (*Mém. de l'Acad.* 1769, p. 465), with regard to this nebula, "On la voit aisément avec une lunette de 8 à 9 pieds, et c'est ce qui a fait penser à M. de Mairan, dans son *Traité sur l'Aurore boréale*, que sa densité varié, parceque, selon un passage de M. Huyghens tiré de son *Système Saturnien*, cette nébuleuse ne se distinguoit bien de son temps qu'avec de très grandes lunettes, comme de 20 ou de 24 pieds de foyer." It is well to bear in mind that Le Gentil distinctly says, in his memoir, that he saw the nebula very differently, according to the telescope employed.

Messier's drawing in his catalogue (*Mém. de l'Acad.* 1771, pp. 485-461) is very satisfactory; and he gives both a *scale* and *catalogue* of the principal stars in and near the nebula. He says of the nebulous star below the great nebula (p. 452), "sa lumière est pâle, et il sembloit qu'un broillard la couvroit." What does Mr. Taylor ("hazy or not hazy") think of the uniform correspondence of "nebulousity" and "haziness" in the usage of astronomical observers, besides the absolute identity of their *literal* signification.

Sir J. Herschel's drawing, in his *Astronomy*, is a reduction of a drawing published in the second volume of the *Asi. Soc. Mem.*, and the epoch is 1st Feb. 1824, and not 1833.

A good deal of the apparent discrepancy between his drawing and that of Rondoni arises from this, that Sir John's drawing is reversed as to top and bottom, just as in the ordinary telescope, but not reversed as to right and left, which is the case in the *front view*. When this is altered, the drawings put parallel, some allowances made for the superior light of Sir J. Herschel's telescope, and considering that neither had the convenience of an equatorial movement, there is not so very much difference, except, indeed, that the second bay, which Sir J. Herschel calls the *sinus Gentilii*, from its discoverer, Le Gentil, does not appear in Rondoni's drawing. Sir

arises how far the apparent discrepancies are owing to actual physical changes in the nebula itself, or if they be owing to the imperfection of the instruments used, or of the vision or powers of accurate delineation possessed by the observers. That it is owing, in some degree, to physical changes is rendered probable, from alterations which have been seen to take place in the last three years.¹⁰ Fortunately, the Roman astronomers have hit on a means effectually to prevent future mistakes of vision or delineation. They have brought the daguerrotype to bear on the object, and, throwing the photographic image of the nebula and its stars on a lithographic stone, have, by an ingenious invention of the Signor Rondoni, which is still kept secret, fixed it there.¹¹ From that stone they have been able to take impressions on

John's drawing is more like Messier's than like Rondoni's. A new drawing may be expected from our illustrious countryman shortly.

And here I may venture to suggest, as the subject of figuring nebulae is somewhat novel, that a previous insertion of stars, from measurement, would add very much to correctness in the drawing, and that faint lines of right ascension and declination, or at least marks on the edge, by which these may be inserted, would aid those who, without a telescope, try to reconcile different authorities. I cannot find any scale for Sir J. Herschel's drawing, or for that of Rondoni, except by reference to a previous memoir. The magnitudes of the neighbouring stars are not given in De Vico's Catalogue of 26, so that there is some difficulty in identifying them in the drawing, and the direction of the meridian is not marked. Messier's drawing and catalogue are more intelligible. These things may be self-evident when the object is before you; but hours are wasted, owing to such trifling omissions, when authorities are compared in the closet.

¹⁰ This is a point which De Vico leaves to time and the consideration of the learned. Mr. Taylor cuts the knot at once.

¹¹ I now come to the *photographical* discoveries of Mr. Taylor. In De Vico's Memoir, opposite page 24, there is a lithographic drawing of the great nebula of Orion, rather coarse and exaggerated, and blurred. Under this is written,—

"Rondoni dis. dal vero, Roma 1841,

NEBULOSA D'ORIONE,

Osservata verso Ovest col telescopio di Cauchoix nella Specola del Collegio Romano."

This is a *second* drawing, differing from one in the Memoir for 1839 considerably.

Opposite page 27 is a much more effective lithograph, with the following legend:—

"Fr. Rondoni dis. dal vero è trasporto il disegno in pietra col Daguerrotipo, Roma 1841,

NEBULOSA D'ORIONE,

Osservata verso Ovest col telescopio di Cauchoix nella Specola del Collegio Romano."

De Vico gives the following account of this last drawing, the *third*:—

"Era già compiuto il superiore disegno, alloraquando piacque allo stesso litografo Sig. Rondoni farmi avvertito di un suo utilissimo ritrovamento. Ciò era, che potosi egli in animo di ottener sulla pietra ciò che il Sig. Daguer scoperse di poter fare sulle lamine inargentate e preparate a tal uopo, v'era infine riuscito: sicchè preparando egli a suo modo alcuna pietra litografica, v'avrebbe in poco d'ora ottenuta l'immagine di alcun oggetto impressavi della

paper, unlimited in number, of singular beauty and of perfect precision; each star, each filmy nebulous streak, faithfully depicting its own position. The scale is large, proportionate to the magnifying and light-collecting powers of the specula¹² employed; the effect is wonderful, and is heightened by being thrown on a beautiful deep azure ground. A globe must have upwards of fifty yards diameter, equal to the width of our Exchange area, to have room for so large a representation of the nebula in question.¹³ The same process has been applied, and with equal success, to the nebula in the girdle of Andromeda. Altogether it is a discovery of the highest importance to astronomical research.

"The account of the labours of the Roman and Neapolitan astronomers upon these different objects is highly interesting, as an example of successful care and diligence. Besides a mass of lunar and planetary observations, made with micrometrical accuracy, and those on the falling stars, for the purpose of ascertaining the difference of the longitude of their two observatories, the Signor De Vico has drawn up a table, by micrometrical measurement, of the apparent right ascension and declination of twenty-six stars encircling the double star Theta, in the nebula of the Sword of Orion, and contained in a space hardly exceeding that of half the apparent disk of the sun. This table, combined with the corresponding photographic portrait, will detect any

luce, con questo sommo vantaggio di poter da essa ritrarne alla maniera solita de' litografi tanto numero di copie, quanto se ne può aspettare da qualunque altro disegno fatto a mano in sulla pietra. In prova di ciò egli m'offerse di farne di presente la sperienza sopra la nebulosa d'Orione; che riuscita felicemente, rese adorna la presente memoria del secondo disegno della medesima che qui riportiamo. Checchè si voglia pensare di questa invenzione utilissima del Rondoni, come dapprima non ebbi fondamento bastevole da rivocarne in dubbio la verità, così ora saputine per gentile comunicazione gli arcani modi non dubito d'asserire, ch'ella potrà sortire un felicissimo esito tosto che ne sia con ulteriori prove alquanto più perfezionato il processo della operazione. Del resto piacque il lavoro agli occhi del Signor Arago che n'ebbe i primi esemplari, e ne partecipò la notizia alla R. Academia delle Scienze in una delle consuete sessioni tenute nello scorso anno. Noi frattanto differiremo a miglior tempo il parlarne più alla distesa; e avvertiremo soltanto che attesa la maggior finezza di lavoro, che con questo nuovo metodo evidentemente si ottiene, si è giudicato spediente l'adoperarlo anche per l'altra celebre nebulosa di cui ora entriamo a favellare, cioè per la Nebulosa d'Andromeda."

There needs no comment on the discrepancy between the text and the interpreter (see p. 32).

¹² "Specula" never means "object-glass" in ordinary astronomical language, but refers to *metallic* reflectors only.

¹³ I do not know whence Mr. Taylor derived his scale, but it is not far from the truth. Its magnitude would have opened the eyes of a person acquainted with the principle of Daguerre's process. How could any one believe that a *magnified* drawing of "filmy nebulous streaks" could be fixed *from the heavens* on a material like stone, and that, too, with a telescope mounted on a Cauchoix's stand without clock-work! Mr. Taylor's credulity at Rome is almost as remarkable as his scepticism at Ormskirk; but then, a copy of the memoir had been "transmitted to him with the greatest courtesy."

changes that time may effect in that which has been justly styled the "transcendantly beautiful Queen of the Nebulæ."¹⁴ By using a magnifying power of 824 on some nights of extreme purity of atmosphere, Signor De Vico has also succeeded in resolving the nucleus of the nebula of Andromeda into a number of luminous points, equal in splendour and very close to one another. He promises to give hereafter the positions of some of the principal of the great number of exceedingly minute stars scattered over this nebula, which since the year 1612, when it was first observed by Simon Marius, has engaged so much the attention of the astronomers. Neither Marius, nor Messier, nor Le Gentil, could discern any star in it, and even Sir John F. W. Herschel could not recognise 'the slightest appearance to give ground for a suspicion of its consisting of stars.' But a happily constructed telescope, with a purer sky, has led the Roman astronomers to a different result. Here, again, the daguerreotype comes with powerful aid to assist their investigations, and numerous minute stars are seen distinctly sprinkled over the beautiful photographic portrait.¹⁵ So far may be considered

¹⁴ The "mass of lunar and planetary observations" consists of eight *differences of longitude, computed from lunar transits* in 1822, mere results, in which there are errors of $-12^{\circ}4$ and $+13^{\circ}7$ from the mean.

The complete lunar transits for 7 days in March 1829, made at Naples.

The complete lunar transits for 5 corresponding days, at Rome.

These have their accompanying stars, and are, therefore, *observations*; but they do not appertain to Father De Vico at all, nor to the astronomer Nobile, but to their predecessors, Dumouchel and Brioschi.

There are no planetary observations in this portion of the Memoir; and the words "micrometrical accuracy" are inserted as "having a good sound, and rounding a sentence well," but of no meaning at all.

The observations at the Collegio Romano of the falling stars for two nights are given in one page. This is a specimen. It contains the clock-time of extinction, and the two stars which define the apparent track of the *bolide*.

The Catalogue of twenty-six stars was observed differentially, each being compared three or four times with one of the component stars of β Orionis. Most probably the places are good, but the "famous telescope of Cauchoux" is not well adapted to differential observations, being on his usual mounting, not differing greatly from that known as Varley's stand in this country. The focal length is 7 feet 4 inches, the aperture 6 inches 3 lines.

[I cannot get the fundamental right ascensions of β^1 and β^2 Orionis to agree with those assigned by De Vico. I presume by *apparent* place, Jan. 1, 1842, he means what we usually call *mean* place, since an *apparent* place to a *given* date would cause unnecessary trouble, and require two computations instead of one. From Piazzi I find the *mean* places for January 1, 1842, by his precessions:—

	Piazzi.			De Vico.		
	^h	^m	^s	^h	^m	^s
β^1 Orionis	5	27	30.8	5	27	36.00
β^2 —	5	27	37.20	5	27	42.24

There is no proper motion, as Airy's place of β^1 (5 obs.) is $5^h 27^m 30^s.83$.

The correction to the *apparent* place is $+2^s.28$ which does not account for the difference. To reduce De Vico's Catalogue to the *mean* places for Jan. 1, 1842, subtract $5^s.11$ from all the right ascensions.]

¹⁵ Opposite page 29 is a very pretty drawing of the nebula of Andromeda, and one which excites curiosity as to Rondoni's process. I need not say the

as the first part of this most important memoir. The sequel contains observations on the ring and satellites of Saturn;¹⁶ and those by which the time of the rotation of the planet Venus on its axis has, at

daguerreotyping saves the redrawing on stone from the sketch. After giving a short history of the nebula, De Vico refers particularly to Le Gentil's account, who seems first to have remarked its true form:—

“Anzi nel centro medesimo comincio (Le Gentil) a discernere quella maggior copia di luce, che in piccoli cannocchiali sembra essere un così chiaro nucleo da gareggiare con una stella. Insomma la descrizione ultima ch'egli ne fece, è un assai fedele ritratto della nebulosa quale al presente anche noi la veggiamo continuamente, e quale ce la rappresenta il disegno che qui ne diamo. E da notare che in alcune notti di rara purezza con un forte ingrandimento di 824 volte il suddetto nucleo ci appare chiaramente risoluto in più punti luminosi di splendore eguale e vicinissimi l'uno all'altro.

“Tra le moltissime e minutissime stelle, di cui questa nebulosa va adorna, abbiamo trascelte alcune delle principali, delle quali in altra circostanza daremo la posizione.”

Besides Mr. Taylor's *photographical* mistake, he misunderstands De Vico in this: the “punti luminosi” are not the same as the “minutissime stelle” which every body has seen. I refer Mr. Taylor to the drawings in Herschel's *Astronomy* and Smyth's *Celestial Cycle*. In no case do I believe these stars to have been carefully laid down; they are not immediately identifiable on comparing the drawings. When De Vico's Catalogue appears it will be very useful, especially if the magnitudes be given even approximately. As the nebula of Andromeda has not as yet been resolved by any other telescope, some uncertainty must attach to the Roman observation, until it is confirmed or the observation frequently repeated. Sir J. Herschel says (*Ast. Soc. Mem.* vol. ii. p. 490), “Its nebulosity is of the most perfectly milky, absolutely irresolvable, kind, without the slightest tendency to that separation into flocculi above described in the nebula of Orion, nor is there any sort of appearance of the smallest star in the centre of the nipple.” It is said, too, not to have been resolvable by the Earl of Rosse with his reflector of 3 feet aperture.

¹⁶ The sequel contains no observations (in the ordinary astronomical sense) of the satellites of Saturn, but only notices of some interesting appearances, viz. that the body of Saturn is not in the centre of his ring, that for several nights a bright speck like one of the least satellites seemed to adhere to the edge of the ring on the eastern anse, and that the outer ring has clearly the subdivision of Encke.

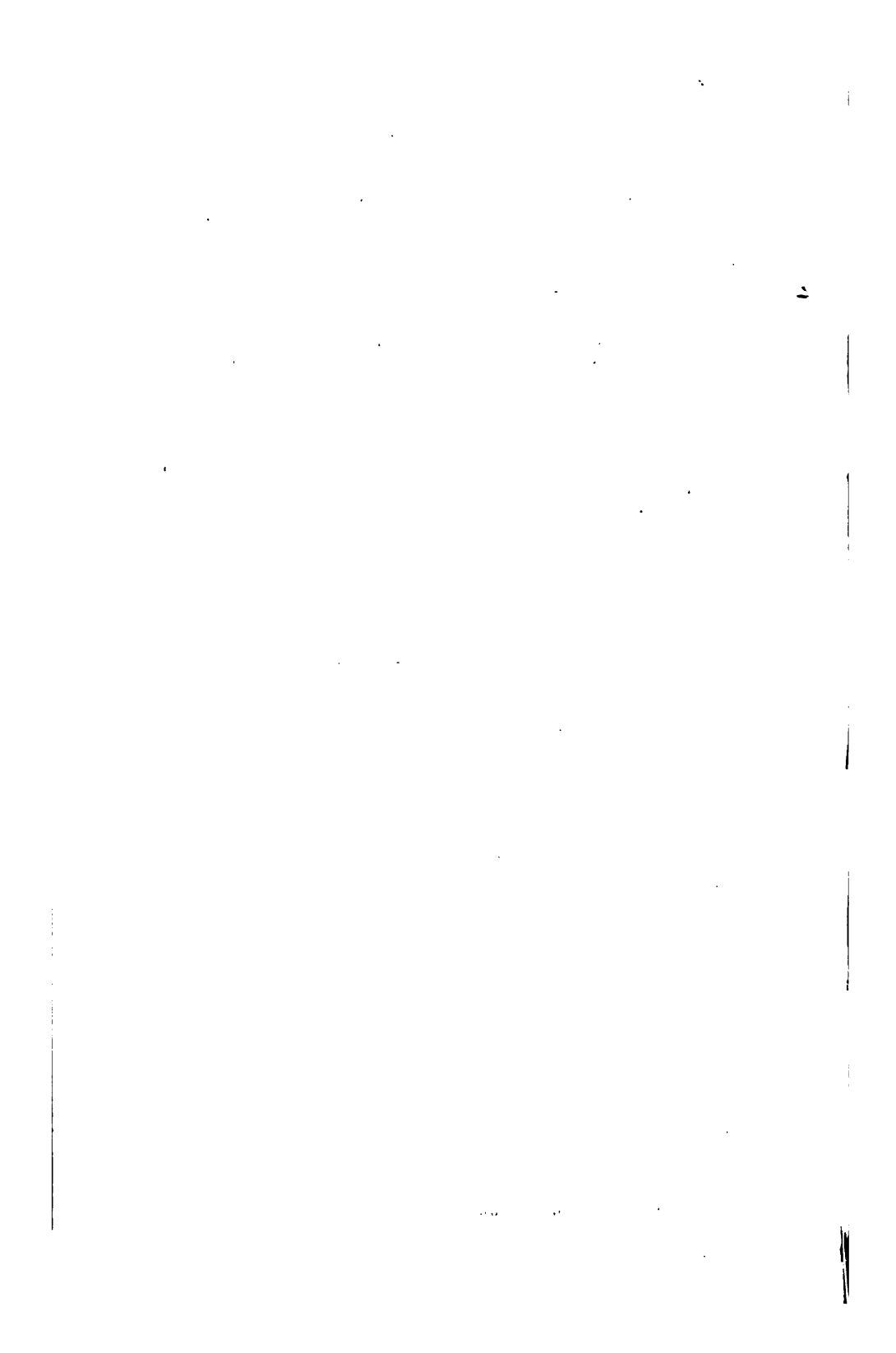
It is only in the remaining and principal part of the Memoir, on the “rotation of Venus,” that there are, properly speaking, any planetary observations. Mr. Taylor, in his zeal to requite the unlucky foreigner who noticed him, has quite misunderstood the meaning of the Memoir. It is a popular account of the objects which have occupied the Roman astronomers for the years '40 and '41, with a few results, the part which refers to the rotation of Venus excepted, which is more complete.

It may seem that I have taken unnecessary pains in dissecting Mr. Taylor, but I wished to point out the mischief of such persons interfering in scientific matters at all, even when there is no wish to be offensive. Soon after the publication of Mr. Taylor's letter, an account of it appeared in a London newspaper. Astronomers who knew something of photography, merely laughed at the announcement as something incredible and most improbable, if not impossible. It was a mere chance, the being asked by our assistant-secretary to translate the title or the legend of the plate, which induced me to look at the original and thus discover Mr. Taylor's mistake. As

length, been determined, and the spots of its disk correctly delineated. Ninety-one designs, on a small scale, of the appearance of the spots on the disk of Venus, taken at various times, are annexed to the present memoir, and a regular map, on a large scale, is announced for the next publication. "T."

"Liverpool, July 30th, 1842."

very few persons trouble themselves about foreign scientific memoirs, especially when the contents are supposed to be absurd, it was exceedingly probable that De Vico's fair fame would, in this heretical land, have been seriously and permanently injured, by having a notorious absurdity fathered upon him. I am not sorry that Mr. Taylor, by forcing publication upon me, has enabled me thus to clear off the imputations which his ignorance had cast on the respectable director of the *Specula Gregoriana*; otherwise I should have been satisfied to laugh at the Liverpool translator with half-a-dozen acquaintance. The prints taken from the daguerreotyped lithograph are very striking, and it is desirable that Signor Rondoni's process should be known.



SUPPLEMENT
TO THE
CORRESPONDENCE
RESPECTING
THE LIVERPOOL OBSERVATORY,
BETWEEN
MR. JOHN TAYLOR, OF LIVERPOOL,
AND
THE REV. R. SHEEPSHANKS.

“Oui, je te soutiendrai par vives raisons, que tu es un ignorant, ignorantissime, ignorantifiant, et ignorantifié, par tous les cas et modes imaginables.”—*Molière*.

SUPPLEMENT, &c.

[N.B. The numbers of reference are to the pamphlet entitled *Correspondence respecting the Liverpool Observatory, &c.*]

THE EDITOR OF THE "LIVERPOOL MERCURY."

I am compelled, sorely against my inclination, once more to enter the lists against Mr. Taylor, or rather, against

"——— *that three-headed canus,*"

of which he, the editor of the *Liverpool Mercury*, and their humble ally, Sir James South, are the component barkers. Each shall be replied to in due order, beginning with the editor.

I charged the editor (see *Correspondence*, p. vi.) with dishonourable conduct: first, because he postponed *sine die* my "moderate" reply to an attack inserted in his own newspaper; and, secondly, because he transferred the MS. to my adversary. To the first charge he has attempted a defence, which I have shewn to be invalid, and which cannot be the *whole* truth. To the latter imputation, which is of more serious import, he makes no reply whatever; at least I can discover none in the following answer to correspondents in the *Mercury* of the 3d of October:—

"THE REV. R. SHEEPHANKS.—This very silly gentleman, whatever he may be as a professional astronomer, has written a pamphlet not at all likely to be purchased by 'a discerning public,' in which bad logic, worse temper, and most untenable assumptions have made him appear more ridiculous than ever, if such a thing be possible. We are so completely occupied about useful matters, that we can hardly promise ourselves the amusement of noticing him. We should, however, like the fun—nothing better; and if we do indulge in such holiday sport, we will prove that, in the adjectives herein used, we have not at

all disparaged him.¹ Mr. John Taylor has sent us a letter in honour of him, which is passed for insertion. As regards the two disputant

¹ I will grant, for the moment, that all the editor's adjectives are true, and that he will be able to prove them so when he is "less occupied in useful matters." What is the conclusion which naturally follows the admission? I can only arrive at this,—that, while he confesses the fault, he denies the existence of the palliating temptation. If he felt that my "moderate" letter was *unanswerable*; friendship for Mr. Taylor, dislike to the ridicule which must attach to Mr. Taylor's *prôneur*, pride in the scientific reputation of Liverpool and its *Mercury*, all or any of these feelings would account for, and, *perhaps*, to a man of no very nice honour, partially *excuse* his swerving from a straightforward course. The *fraud* cannot be denied; and to defend the *wrong* by calling the injured person a fool, only makes the treachery more inexcusable, because needless. "Masters, it is proved already that you are little better than false knaves, and it will go near to be thought so shortly." When the editor can shew that he has been fair, he may be as "funny" as he pleases; but the integrity remains to be proved, and he is really more concerned in this than in any scientific question whatever. It is very easy to say, "We never published of any man or set of men, what we did not believe to be true; and we have never failed to recall any thing we have said, if error were discovered in it. Whatever we have printed, if we have not immediately and frankly corrected it in every particular, is what, to this hour, we believe to be wholly true." (*Liverpool Mercury*, Oct. 3.) There is not "one halfpenny-worth of" performance, good Mr. Editor, "to this intolerable deal" of vague profession, so far as I am concerned. The admission that I may be something as "a professional astronomer" is singularly at variance with the intention of the paragraph. My dispute with Mr. Taylor arose on astronomical ground, and the really important matter is whether I am astronomically right. For a pretender to *logic* the editor is too fond of a *non sequitur*.

To "our" quality of unimpeachable fairness the editor has, in the same number of his journal, called witnesses; viz. a "saying upon 'Change,'" and an anonymous "friend from a distance." Supposing the saying to have been said, and by a responsible person, and that the "friend from afar" is a true man, and not another form of the "us" or "we," so potent with the vulgar, and so convenient for the ignorant—the editor, if acquainted with criminal practice, must know that when the offence is self-evident and not disputed, character can only be pleaded in mitigation of punishment. In all former instances the *Liverpool Mercury* may, for aught I know or care, have been as pure as unsunned snow; and I am charitable enough to hope that, for the future, it may be a specimen of what an English newspaper ought to be. But this little spot is yet to be washed out. A stranger's "moderate" defence has been rejected; the MS. has been handed over to the assailant without any cause assigned; and the only reparation of the wrong, hitherto, has been a liberal application of epithets, which, in my conscience, I believe the writer *knows* to be false. The editor is wholly ignorant of astronomy and mathematics,—his faith in Mr. Taylor shews this; and he may be incapable of comprehending the nature of proof,—saucy writers often are; but that he really thinks me a "very silly gentleman," or that he has found nothing but "bad logic, worse temper, and most untenable assumption" in my pamphlet, I do *not* believe; and there is no assumption in saying this, because *I happen to know* that gentlemen, with whom neither the editor nor Mr. Taylor would presume to compare themselves, are

astronomers, the stipendiary and the amateur,² we would just ask, what has Mr. Sheepshanks ever done—what has he the ability ever to do—for the service of science, equal to the invention by Mr. Taylor of the perennial celestial globe?³ Do six lines of titles, in close newspaper type, balance this practical good?⁴ We think not."

of a different opinion. Has the editor really read the pamphlet at all? Does he still hold that Mr. John Taylor,—in his prognostics regarding Halley's Comet, in his denial of the appearance of discs in stars when viewed by good telescopes with high powers, in his account of *Photography at Rome*,—has been found to be in the right? What do "we at this hour believe" on these points? Do not "we" rely rather too securely on the maxim "Et apud eos dicimus, qui nesciunt, et ea dicimus, quæ nescimus ipsi?" The ignorance and credulity of the believers in the *Liverpool Mercury* surely have some bounds.

² It would not become me to say "what I have done," or what I "have the ability to do," for the service of science, but the editor has been a little hasty in calling me a *stipendiary*. Astronomy has been to me a hobby, not a horse for hire. If I used Mr. Taylor's fantastic phraseology, I might call it "a passion of my soul." I never held an astronomical situation to which a salary was attached; I never received or expected a shilling either for my trouble or for any expense which my whims have led me into. Mr. Charles Knight presented me with a copy of the *Penny Cyclopædia* as a compliment for my articles on practical astronomy. This is the only thing resembling *payment* I can remember, and it was unexpected.

³ The distinguishing property of the "Perennial Celestial Globe" belongs, as an *invention*, to me, or to the editor, as much as to Mr. Taylor, as I shall shew a little further on. Hence his *minor* being gone (his *major*, viz. the utility of the said invention, is very disputable), the editor's logic halts at starting. I will, however, for the nonce grant him both his premises, and what has the conclusion to do with the subject? I have *proved* that Mr. Taylor is ridiculously ignorant of the language, of the resources, and of the facts of modern astronomy. This result is indisputable: Mr. Taylor himself cannot impugn it; a thousand useful inventions would not alter it. I could wish the editor would be more discreet in his application of the terms "illogical," "assumption," &c. His broadcast style of writing, "though it make the unskilful laugh, cannot but make the judicious grieve."

⁴ The editor opines that six lines of titles do not "balance this practical good." I might say with equal *impropriety* that one pound of pica or longprimer outweighs "our" unimpeachable fairness. There is no proportion between things of such dissimilar nature, one cannot "be multiplied so as to exceed the other." Having shewn Mr. Taylor how to spell the *Connaissance* and Rondoni's legend, to interpret Mr. Dawes's observations and to understand his own, I will venture to give the editor a short lesson on the Science of Inscriptions.

A *well-informed* English gentleman knows that a fellow of Trinity College, Cambridge, is probably a person a little above the average in intellect; that a member of the Board of Greenwich Visitors, and Secretary of the Royal Astronomical Society is in the way of astronomical information; that a writer in the *Penny Cyclopædia* must have had the confidence, to some extent, of the Society for the Diffusion of Useful Knowledge, of Mr. Long the editor, and Mr. Charles Knight the publisher, of that useful work. Hence

I consider the editor responsible for the language of this paragraph, still remaining of opinion, from internal evidence, that he is not the author of the notice to correspondents, p. vi. When he has cleared up the little awkwardness of transferring my MS. from his desk to Mr. Taylor's pocket; apologised for his rudeness, or inattention, or something more, in omitting the publication; relinquished his "us" and "we;" named his name, and told us plainly

"That I, one Snug the joiner, am
A lion fell, or else no lion's dam;"

I will engage to answer anything except "fun," should he love "fairplay" well enough to give me a place in his columns. As these matters of course cannot be refused by any honest man, I must presume the editor will accede to my stipulations. Should he decline them, let us hear no more of his unimpeachable fairness, frankness, and candour; and, above all, let him drop the title of "gentleman,"

"Doff it for shame,
And hang a calf's skin on those recreant limbs."

MR. JOHN TAYLOR'S REPLY.

Having got rid of the editor, or at least postponed our *duello*, till the conditions of combat are agreed on, I now turn to Mr. John Taylor, who shall be allowed to speak for himself, with the advantage of a running comment. His reply, or non-reply, for he seems uncertain which to call it, is thus given in the *Liverpool Mercury* of October 3d, 1845:—

he would *guess* that the writer of the pamphlet, if not an impostor, was more likely to be well acquainted with the subject matter than Mr. John Taylor, of Liverpool, of whom nothing is known, except his own description, that

"In the wide world he is nothing at all."

All these separate probabilities may be small, and their combined weight may fall very far short of certainty, still they are such as no prudent man would decide against without examination. This is precisely what I had in view in "putting on my tail,"—to gain a hearing which my adversaries had combined to deny me. I cannot see why I should amputate it in deference to the bareness of my antagonist, and I merely follow the *custom*, as every body knows. To *weigh titles*, however respectable, against a practical good—against fact or demonstration—is an idea worthy of the "logic" or "fun" of Mr. John Taylor's faithful disciple, the editor of the *Mercury*.

"THE REV. RICHARD SHEEPSHANKS, M.A.,

Fellow of Trinity College, Cambridge;

Member of the Board of Visitors of the Royal Observatory;

Foreign Secretary of the Royal Astronomical Society;

F.R.S., F.G.S., F.C.P.S., &c.;

*Author of the Articles on Practical Astronomy in the Penny
Cyclopedia.*

'Begar! Monsieur Tonson come again!'

To the Editors of the Liverpool Mercury.

"GENTLEMEN,—I am obliged by your transmission of the pamphlet on the 'Correspondence respecting the Liverpool Observatory,' sent to your care for me from London, by Mr. Sheepshanks, my *vernal* antagonist, for you know our controversy began in ARIES (γ), to speak astronomically, if I may be so presumptuous. It is a compound of malignant slander, imbecile rage, and miserable folly, undeserving of any serious reply—nor, indeed, deserving of any reply.¹ I shall, therefore, adhere to the resolution which I expressed in my letter to you of May 13. As Figaro says,—

*'Qu'il fasse des libelles! dernière ressource des laches!
Il n'est plus dangereux; bien démasqué, à bout de voie.'*

"Let him write libels! the last resource of the base! He is no longer dangerous; fairly unmasked, and having exhausted all his means.'²

"To his calumnious accusation of my having in any way hindered

¹ "Tout beau! l'empoiement est fort peu nécessaire;
Et lorsque de la sorte on se met en colère,
On fait croire qu'on a de mauvaises raisons."

I am glad to find that Mr. Taylor is so unconcerned, for I really was rather apprehensive that the effect of my pamphlet would have been antise-dative. The week before its appearance, Mr. TAYLOR REQUESTED my kind friend Mr. Baines to WITHDRAW THE ADVERTISEMENT FROM THE LIVERPOOL TIMES, which Mr. Baines very good-naturedly did, from pity, I believe, to Mr. Taylor's apparent agitation. I wrote immediately to Mr. Baines to express my regret, as the pamphlet was actually in the press. Luckily Mr. Taylor possesses more equanimity than he had led us to fancy, and bears his exposure with the calmness of Sir Fretful Plagiary. To *begin* a long reply by declaring that you will adhere to a resolution of "declining further controversy with such a disputant" (see p. 45), goes beyond Gonzalo's oversight, the "latter end of whose commonwealth forgot the beginning."

² If a libel means a temperate defence of yourself and a simple dissection of your adversary's "most weak *pia mater*," I am guilty of writing libels; but that I have not "exhausted my means," shall be shown presently, to the satisfaction of everybody, save Messrs. Taylor and Co.

the publication of his letters in the *Liverpool Mercury*,* or of having ever written any thing concerning him to which my full name was not attached, I here publicly and solemnly give my denial. I consider Mr. Sheepshanks to have, in this instance, knowingly advanced a deliberate falsehood;† and having said thus much, I dismiss him from my

* Mr. Taylor does not "understand the words of action." The two charges to which he alludes are (see note, p. vii.) my expressed *belief* that he and the editor of the *Liverpool Mercury* held sweet counsel together to stifle my reply, or at least to secure a one-sided hearing; and my attributing the notice to correspondents (p. vi.) to his, and not to the editor's pen, for reasons there stated. On the first point the editor confirms him by a note (which, by the way, is rendered a little suspicious by the self-commendation), on the latter the editor is silent. After hearing both stories, my belief is unchanged. But should this be a mistake, I may, indeed, be guilty of uncharitableness, but I have not "knowingly advanced a deliberate falsehood." I have not asserted a matter of fact, but a matter of opinion, arrived at from admitted data by considerations of probability. Let me shew more fully why I neither believe in Mr. Taylor's whole, nor in Mr. Editor's half denial.

When my Liverpool friends applied to Mr. Baines to do me that justice which was refused by the editor of the *Mercury*, they were informed that Mr. Taylor had been to the *Times* office to make inquiries there *with the MS. sent to the editor of the Mercury in his pocket*. Mr. Taylor being thus detected, informed against himself in a letter which is inserted in p. 22. Neither then nor now is there any hint *how* this letter came into his possession, or *why* it came into his possession. "The copy of a letter" (see p. 22) is the letter *itself* sent to the *Mercury*, and the *naïve* remark that he had got it "only this morning," is very like preferring a defence before accusation.

The intimate scientific connexion between Mr. Taylor and the editor is evident from several circumstances; and is, I presume, so notorious, that neither will attempt to deny it. I do not suppose Mr. Taylor stole my MS.; and it is incredible that it was put into his hands and got into his pocket without some remark or on some understanding. On *my* hypothesis, every thing is simple enough; the injustice of the editor has a motive, as well as their joint fraud. Mr. Taylor and his editor were clearly in a "fix," their shuffling shews that; and, after Mr. Taylor's correspondence with Mr. Dawes, Mr. Lassell, Lieut. Stratford, or myself, there is reason to think that *he* would not stick at a trifle for his deliverance. I have no such *antecedent* reason for thinking harshly of the editor, but I am not *now* satisfied with *his* bare denial. He must first shew *why* he did not print my reply, and *why* he gave it to Mr. Taylor; and Mr. Taylor has the very difficult task of explaining *how* my MS. got into his pocket. Will he say that *he was taking it home to revise and prepare for press*? Until Mr. John Taylor has cleared up this mystery he must not expect implicit belief for any denial, however public or solemn. I shall repeat with Caius, "Vat shall the honest man do in my closet? Dere is no honest man dat shall come into my closet."

I "attributed without hesitation" the notice to correspondents in page vi. to the pen of Mr. Taylor. *I do so still*. The style and temper are precisely those of his short letter at page 45, and of the commencement of the

* "Mr. Taylor never did any thing of the kind. He knows us too well to attempt such a thing, had he even been so inclined.—EDITS."

thoughts, as 'My Uncle Toby' did the blue-bottle fly; and I recommend him, with perfect sincerity, to the tender care of his friends.⁴

"There is a matter, nevertheless, which it is right to clear up, as it is decisive of the trust to be reposed in the assertions of Mr. Sheepshanks. Mr. Sheepshanks quotes Sir J. F. W. Herschel, as having commended an ephemeris for Halley's Comet, published by Lieut. Stratford, the editor of the *Nautical Almanac*, of the accuracy of which ephemeris I had expressed a doubt.

"This is contrary to truth. The ephemeris which Sir John Herschel commended was not published, nor, in fact, calculated, until a number of weeks after my last letter in the London *Times* on the subject of Halley's Comet. The facts were these:—On November 5, 1835, Lieut. Stratford published an ephemeris for Halley's Comet, with the elements on which the computation was said to be made. These were published in the London *Times* of November 10, 1835. On November 14, I wrote a letter to the editor of the *Times*, challenging them as erroneous, which letter was published in the *Times* of November 18, 1835. On this, Lieut. Stratford sent to the London *Times*, under the date of November 25, eleven days after the date of my letter, a *new set of elements*, differing materially from those which he had before published, and which I had challenged, and with only two computed points for a *new ephemeris*. To do this he employed, as he there informs us, 'seven very expert computers nine entire days;' and truly not before they were wanted.

"These *new elements* were tolerably correct, and the positions deduced from them were afterwards extended into a very good ephemeris,

present epistle: violent and singularly inappropriate abuse, much after Cotin's pattern:—

"Qui méprise Cotin n'estime pas son roi,
Et n'a selon Cotin, ni Dieu, ni foi, ni loi,"

which assorts but badly with his pretence to indifference. The confounding my disclaimer of profound mathematical knowledge with a confession of ignorance is Mr. Taylor all over. A conjunction of two such reasoners in one printing-office is as incredible as that two phoenixes should roost on the same perch. Let any one turn to p. 60, and read the address "*To the Rev. W. R. Dawes, Dissenting Minister, Ormskirk,*" and compare that with page 76, and he will see another startling resemblance between the indisputable and hypothetical Mr. Taylor. In both cases gratuitous insolence is followed up by vulgar cant. The editor's pen is ready and flippant, but there is no straining after heavy-shotted abuse; he is, indeed, sufficiently ignorant, but is chiefly remarkable for shallow pertness, such as some "gentlemen of the press" are too ready to indulge in, and from which other gentlemen carefully abstain.

"My Uncle Toby first caught and then dismissed the blue-bottle fly. If Mr. Taylor has caught me, it is as Paddy caught a Tartar, and the dismissal is premature, for here I am buzzing again. I am not well read in Sterne, but surely the character of Smellfungus would sit more naturally on Mr. Taylor than my Uncle Toby's; or if he wishes for a *tableau* from *Tristram Shandy*, what does he say to enacting the part of the meek Dr. Slop when run down by Obadiah and the coach-horse?

which ephemeris was with justice praised by Sir J. F. W. Herschel in the month of October 1836, nearly twelve months after my correspondence.⁵

⁵ We now get upon sounder ground. Mr. Taylor having carefully perused ninety-one pages of libel, which he scorns to reply to, selects two points, and, lest these should not hold and his gaskins be in danger, brings forward a third, as a new claim to astronomical reputation,—

“ Nasidiene, redis mutatz frontis ut arte
Emendaturus fortunam.”

Let us see whether he is more lucky this time.

“ Mark now how a plain tale shall put you down.”

First, let me beg my reader to read carefully over the Appendix, No. I., pp. 51-57; and then to follow me critically and even suspiciously while I analyse and complete Mr. Taylor's story.

Mr. Taylor's statement, from “On November 5, 1835,” to “nine entire days,” is moderately correct, though I may remark that he only mentioned a discrepancy between the elements of Pontécoulant and Rosenberger and those of Lieut. Stratford, which is not tantamount “to challenging” the latter “as erroneous,” nor any thing like it. I am also inclined to think the discrepancy to which he alluded arose from his confounding the two modes of stating the longitude of the perihelion and the inclination of the orbit. The next paragraph is possibly true, but so loosely worded that I do not quite understand what Mr. Taylor would be at. The *new elements* were more than *tolerably correct*, for they generated “the *very good ephemeris* which Sir J. F. W. Herschel praised with justice.” What Mr. Taylor means by saying “the positions deduced from them were afterwards extended into a very good ephemeris” is not clear; and the change of epithets from “*tolerably correct*” to “*very good*,” would imply, in a logical writer, some change or improvement *in transitu*. No such change took place, and the ephemeris is merely the expression of the elements in a form fitted for observation. Both must be good or bad together.

Having got thus far, Mr. Taylor glides away from the topic as if it were exhausted, whereas, so far as I am concerned, it has not yet been entered upon. Why did he not go on to say, as he might have done with perfect veracity,—

“On December 1st I replied to Lieutenant Stratford's letter of November 25, which letter, as I have stated above, contained ‘a new set of elements,’ from which ‘the very good ephemeris’ was computed, and to the following effect:—

“First, I declared that ‘Lieutenant Stratford was pursuing an erroneous method which could never give even moderately satisfactory results,’ and that ‘no elliptic orbit could fulfil the places of the comet.’

“Secondly, I recommended, as the only mode to ‘designate the motion of the comet,’ a series of parabolic sectors, ‘each sector being founded on three good observations;’ but I did not suggest any way of predicting a place for the future, *i.e.* of making an ephemeris.

“Thirdly, I declared that to make any attempt to apply the elliptic hypothesis to a body so solicited as Halley's Comet was worse folly than attempting to square the circle, which was typified by the ancient mythologists in the fable of the Danaides.

“Fourthly, I confounded the ‘method of least squares’ with dividing a circle into a number of small squares like a sieve, and considered it to be an

"As to my having objected to the method of 'least squares,' and to the monstrous labour and uncertainty of the methods pursued by Lieut. Stratford, however commendable his diligence, my justification shall be the following extract from a posthumous work by Laplace, which I did not, however, see till long after.⁶ If I erred, although I

old invention, which would be laid aside for some other antiquity under a new name.

"Fifthly, I expressed my opinion 'that if Mr. Stratford would give to the public my parabolic sectors he would do real service to science;' but that 'the immense calculations which he has enumerated were all labour spent in vain, as the result has shewn, and would further shew;' and I, moreover, declared that any elliptic orbit 'would fall woefully from the truth when compared with the observations in the ensuing spring.' I must admit that these opinions were written *in answer to Lieut. Stratford's letter of November 27th*, and that the very good ephemeris praised by Sir J. F. W. Herschel was the result of these immense calculations; but I still think proper to charge Mr. Sheepshanks with wilful falsehood because he has laughed at my prophecies, and brought Sir J. F. W. Herschel's authority to prove their folly.

"Sixthly, I prophesied 'that good luck as well as skill would be needed to come within a degree of the comet's position in March and April,' and appealed to time 'to shew how far I spoke without knowledge;' and I now acknowledge that 'the new elements' which were obtained by Lieutenant Stratford's immense calculations, and which I did deride and ridicule as aforesaid, gave the places of the comet in March and April so closely that Sir J. F. W. Herschel's observations scarcely came closer, and I cannot deny that Mr. Sheepshanks' remarks apply to this specimen of my prophetic powers, for page 56, notes 15 and 17, are clear evidence of the fact; but I will not, therefore, admit that I was wrong or a sorry prophet."

If Mr. Taylor had continued his tale to this point he would have been a fair and candid historian; but such a simple story would not suit his purpose. He attempts to apply my remarks, *which are pinned upon his letter of December 3d by numbers and references*, to his letter of November 14th, and tries to persuade his incautious readers that his objections were made to an earlier ephemeris, and not to Lieutenant Stratford's "methods" and "new elements." Let any one read the story as I have related it in my pamphlet, and the *impudence* of Mr. Taylor's defence will appear absolutely startling; such as to justify me in withholding my belief from any thing which he states on his own authority on any occasion whatsoever.

⁶ In the preceding note I have touched on Mr. Taylor's knowledge of the "method of least squares." He did not know, and does not know, the meaning of the words any more than a parrot. He now cites a passage from Laplace, which he understands just as little, to prove that in certain cases the method of least squares is not so good as some modification of it due to Laplace himself; and he concludes by saying *very modestly*, that if he erred he erred with Laplace and Poisson.

Mr. Taylor *never objected to the method of least squares at all* on the ground of its being *inapplicable to the problem*: he maintained the impossibility of calculating the motion in any ellipse whatsoever, and recommended a series of parabolic sectors in its stead. He has now found out that 'comets of which the periods are known are in effect planets,' which is just the contrary of his celebrated letter, and, in fact, an admission that every thing he then said of parabolic sectors, &c. is unmixed nonsense. Will Mr. Taylor shew where Laplace and Poisson have confounded the method of "least squares" with "a

have yet to learn that I did, I erred in good company, and need not to be ashamed of it:—

“ ‘When it is intended, for example, to correct the elliptic elements of the movement of the planets (*comets, of which the periods are known, are in effect planets*) equations of condition are formed, in making equal each observed longitude, to the longitude calculated by those

circle divided as well as it may be into small squares?” for that is what he has got to prove. That in some cases the simple method of least squares is not the best, may be taken on Laplace's authority: but to apply this dictum, it is for Mr. Taylor to prove that it was not the best, when the elements of Halley's Comet were to be determined with the data at Lieutenant Stratford's disposal. If Mr. Taylor cannot do this, Laplace and Poisson are quoted upon the Pecksniff principle of “using good, sounding words,” and in confident assurance that the readers of the *Mercury* are no better reasoners than its editor. That the method of “least squares” succeeded is certain, for the “beautiful ephemeris” lauded by Sir J. F. W. Herschel is the result; and until Mr. Taylor enlightens the world with a popular commentary on Laplace's “general expression of the most advantageous factors,” most of us will believe that it was properly applied.

I have asked one of the *very* few persons who have mastered the *Théorie Analytique des Probabilités* (in this country at least) what is the meaning of Laplace, and I here give his reply. “What is commonly called the *method of least squares*, supposes that all the observed quantities have their *weights*, as they are technically called, equal. When different probabilities of error are introduced, an extended method must be adopted. Laplace, who did not consider this extended method in his work, gives it in the third supplement. This extended method is really one of least squares; for it consists in making the sum of certain *quadratic* expressions a *minimum*. When a mathematician extends a method, he has his choice either to extend the name of the old method, and make it include every thing, or to let the old name denote the old method, and find a new name for the extension. Laplace, as appears by the passage which Mr. Taylor has quoted, has condemned the old method in certain cases, calling it the method of least squares; and has referred the reader to the third supplement for his new method, to which, however, he gives no new name. As in hundreds of other cases, he cares nothing for a looseness of language in his descriptive paragraphs which he knows a proper look at his symbols will set right. But could he have had the least idea that a person who can only read his words, and not his symbols, would have looked at his descriptive paragraphs with a view of making him speak either one opinion or another, he would have written a few sentences in mercy and in prudence to prevent his would-be reader from compromising both himself and his author.

“It is dangerous for a person who is not a mathematician, to try to read paragraphs descriptive of symbolical processes. He resembles the man who read his *Euclid*, ‘leaving out the A's, B's, and C's, and the pictures of scratches and scrawls.’

“This extension of Laplace cannot be used without knowing the relative probabilities of error—precisely the thing which can hardly be known. Until these relative probabilities are known, the common rule is that of sound judgment, be the relative probabilities what they may.”

So much for Mr. Taylor and the method of least squares; the companionship he claims with Laplace and Poisson only reminds me of a most unsavoury, but very striking similitude.

elements augmented, each, by its own correction. A great number of equations of condition are thus formed. Afterwards, each of them is multiplied by the coefficient of the first correction, and all those equations, thus multiplied, are added up, which gives a first final equation. By working in the same manner, relatively to the second correction, to the third, &c., as many final equations are formed as there are corrections determined in resolving those equations. *But the longitude is not the result of one direct observation; it is deduced from two observations made with different instruments, of which one gives the right ascension of the star, and the other gives its declination.* The law of probability of the errors of each of those instruments may not be the same; moreover, those errors have, according to the position of the star, a different influence on the longitude. THE METHOD OF LEAST SQUARES, OF WHICH SEVERAL GEOMETERS HAVE GIVEN SOME VERY UNSATISFACTORY PROOFS, DOES NOT HERE GIVE THE MOST ADVANTAGEOUS FACTORS; IT HAS ONLY THE ADVANTAGE OF OFFERING A REGULAR MODE OF FORMING THE FINAL EQUATIONS. I have presented, in the third supplement to my *Analytic Theory of Probabilities*, the general expression of the most advantageous factors.—Laplace, *Supplement to the 5th Volume of the Mécanique Céleste*.

“On this subject, I have to add the authority of M. Poisson to that of Laplace. Those two I judge to have been sufficient for my justification at that time—ten years ago, whatever improvements in those calculations may have been made since.

“Having taken up so much of your room, I hesitate to ask for a few lines more,—yet, as I conceive the interests of science to be involved in what I ask, I will venture to do it. Mr. Sheephanks has suppressed the concluding paragraph of my letter to Lieut. Stratford, of December 3, 1835. It is as follows, and I should wish it to appear, that the notions then stated may be again laid before the public:—

“‘I shall conclude with stating, by your permission, one view of the theory of Halley’s Comet, applicable also to the orbits of other comets, which I consider to be important, and which has not hitherto been noticed by any one of the writers on the subject. The perihelion point of Halley’s Comet has had a retrograde movement, in reference to the order of the signs of 194 minutes, measured on the ecliptic, since the year 1456, the earliest epocha for which the elements of its orbit have been ascertained. If this be taken as the mean movement of the apsides of the orbit, the line of its great axis was vertical to the line of the nodes about 2400 years ago; and what is more interesting, Halley’s Comet, in its 76 years’ revolution, must have crossed the path of the earth round the sun about 1800 years ago, at as small a distance as Biela’s Comet now does in its $6\frac{1}{2}$ years’ revolution. The movement here stated has reference to the moveable ecliptic; the absolute yearly motion of the apsides, or extreme points of the comet’s orbit, in space, would be only, on the same average, 20 seconds, which requires 64,800 years for the whole circle. The change which is thus gradually taking place in the direction of the great axis of the orbit will account for the diminution observed in the splendour of the comet since the time

of its recorded appearance 2200 years ago, at the birth of Alexander the Great, and afterwards at the birth of Mithridates. At that time the comet, in its descending node, passed much nearer to the path of the earth than it now does.⁷ The movement in question is analogous

⁷ Having disposed of Mr. Taylor's attempt to defend the absurdities which were already exposed (will he venture upon them again?), I come to new matter which was passed over in the pamphlet, p. 57, as being "vague speculations on the changes of the perihelion of the comet, its causes and consequences." Mr. Taylor says, incorrectly, I *suppressed* the paragraph, which, perhaps, "in N. Baily's very good dictionary," is the meaning of *omitted*. My readers will see, when I have examined the paragraph a little more closely, whether "the interests of science" are involved in the matter, and whether the opinions which he again lays before the public are any thing but idle and incorrect twaddle, unworthy of serious notice.

Astronomers have not speculated hitherto on the *secular variations* of the elements of cometary orbits, and for an excellent reason, nothing is known with regard to them from observation, and the theory presents enormous difficulties.

Every body who talks of comets, except Mr. Taylor, knows that good elements of a comet's orbit could not possibly be ascertained from observations made in 1456, and that a handful of minutes before the time of Tycho was a mere trifle in practical astronomy. This palpable ignorance is discreditable to a man who professes some acquaintance with the history and literature of astronomy. The way in which Pingré identified the comet of 1456 as Halley's Comet was by *assuming* the elements and changing the time of perihelion passage until the orbit represented the observations. Mr. Taylor may, perhaps, doubt my word, so I give him, in addition to what is implied by Pingré, *Cométographie*, vol. i. pp. 459-64, the authority of M. Laugier, *Additions à la Connaissance des Temps*, 1846, p. 99. "De 1835 à 1531 les observations étaient assez exactes pour qu'on ait pu calculer l'orbite; mais, pour montrer que la comète de 1456 était une apparition de la comète de Halley, on a été réduit, faute d'observations précises, à CALCULER D'APRÈS LES ÉLÉMENTS CONNUS DE CETTE COMÈTE SA TRAJECTOIRE APPARENTE POUR 1456, et l'on s'est assuré que la route indiquée par les historiens contemporains coïncidait à-peu-près avec la trajectoire calculée." Mr. Taylor, after verifying this passage, and referring to Pingré, will be pleased to admit, that in talking of *elements ascertained in 1456*, he has talked after his fashion, *i.e.* ignorantly. M. Laugier, by assuming the elements, identifies Halley's Comet with one seen in 1378, the observations of which are taken from the Chinese historians. In that year the comet approached, on October 3d, within 0.15 of the earth (Mr. Taylor may multiply this decimal by the millions of miles in the earth's mean radius, and so give himself the air of a *discoverer* to the readers of the *Liverpool Mercury*, and its accomplished editor). After stating the intervals between the several observed perihelion passages, of which the extremes are 77.58 and 74.91 years, M. Laugier concludes by saying: "Nous connaissons maintenant sept apparitions de cette comète: en remontant dans les anciens temps, l'astronome ne rencontre plus que des indications trop vagues pour les soumettre au calcul; il doit par conséquent conserver peu d'espoir d'y retrouver d'autres apparitions de cet astre qui, depuis Halley, fait partie de notre système." In the same volume of the *Connaissance*, pp. 69-84, is a memoir by M. Edward Biot, on the apparition of comets mentioned by the Chinese historians, *which may have been the comet of Halley*; but he considers this

to that of the perihelion point of the earth in the ecliptic, only that the movement of the earth's perihelion is conformable to the order of the

tracing backwards to be hopeless beyond the Christian era. M. Laugier's remark with respect to the ancient observations must not be extended to the *Chinese* observations which M. Biot has lately brought forwards.

Of the elements of the orbit determined by Halley at the apparition in 1531, Pingré says :—" Ces observations d'Apien sont assez grossières, soit à cause de l'imperfection des instrumens dont il se servoit, soit parce que la méthode qu'il employoit pour déterminer le lieu de la comète, étoit, sur tout alors, fort équivoque dans la pratique. Ainsi la théorie de l'orbite de cette comète, deduite des observations d'Apien et calculée par Halley, n'a pas toute la précision qu'on pourrait désirer." There are, in fact, no accurate elements to be got from observation before the apparition of 1682.

Mr. Taylor's foundations being thus annihilated, I suppose the superstructure must follow. Indeed, as he neither uses the proper terms of art, nor refers to authorities, nor explains his procedure, it is rather a guesser of riddles, an Œdipus, who is needed, than an astronomer, to interpret him. I can only do so conjecturally, and with moderate confidence that I am on the right scent.

The "important view," "hitherto not noticed by any one of the writers on the subject," seems to be this. Supposing a continued motion of rotation of the apsides of the orbit, its intersection with the ecliptic may at some time have been such, as to have brought the comet in its descending node nearer to the path of the earth. To make this a little clearer than Mr. Taylor has done: take an ellipse representing the orbit of Halley's Comet. With the sun as centre, and the mean distance of the earth as radius, draw a circle which will cut the ellipse in two points. From each of these points draw a line through the focus of the ellipse to the curve, and when either of these lines represents the line of nodes, Halley's Comet will cross the earth's orbit very nearly. Mr. Taylor has not expressed himself quite so much to the purpose, for he has muddled himself with making the line of the great axis "*vertical*" to the line of the nodes about 2400 years ago, and brought in Biela's Comet as an illustration, by which nothing is illustrated. But this is evident, that if the apsides of Halley's Comet (or of any other comet, having a perihelion distance not greater, and an aphelion distance not less, than 1), do perpetually move *backwards* or *forwards*, measuring from the nodes on the orbit, the time has been (and will come again) when the path of the comet nearly touched the path of the earth, which is all that we know at present on the matter, and which is as much a discovery as that 6 and 4 make 10.

There are two methods of stating the longitude of the perihelion. One is to reckon it on the orbit from the ascending node (Rosenberger has done so); the other is to reckon it along the ecliptic, from the mean equinox of the date, in the order of the signs, to the node nearest perihelion, and so from that node along the orbit to perihelion. This is the usual mode, and that followed by Lieut. Stratford in the "new elements." A third method has, I believe, been employed, viz. to give the longitude of the perihelion *projected on the ecliptic*; but this, though most easy of comprehension, is not convenient for calculation. To illustrate Mr. Taylor's "*important view*," and to try his "opinions" upon our present data, I proceed to find the longitude of the perihelion from the descending node, reckoned on the orbit, at the respective apparitions. The process is simple enough: either take the supplement of the longitude of the perihelion in the first mode of reckoning, or subtract 180° + longitude of the ascending node from the longitude of the perihelion in the second mode, when the result will

signs, while that of the comet's perihelion is contrary, the comet's motion being itself retrograde, and that of the earth direct. Both

be the angle between the major axis and the line of the descending node. Adopting the elements of the catalogue appended to Olbers' celebrated tract "*On the easiest and most convenient Method of computing the Orbit of a Comet*," &c. Weimar 1797;—of Rosenberger's papers in the *Astronomische Nachrichten*, Numbers 196 and 180;—and of the *Nautical Almanack* for 1839 (or what is the same thing, the *new elements* held so cheaply by Mr. Taylor on December 1, 1835, and esteemed by him so highly now), I find:—

Data.	Elongation of Perihelion from descending Node.	Difference.	Authority.
1456	72° 30' 0"	−0° 16' 0"	Pingré, assumed.
1531	72 14 0	−0 19 0	Halley, from Apian.
1607	71 55 0	−1 1 56	Halley, from Kepler, &c.
1682	70 53 4	−1 30 34	Rosenberger.
1759	69 22 30	+0 1 18	Rosenberger.
1835	69 23 48		Stratford's "new elements."

It is only the three last lines which are really of authority; and I think there are some small corrections yet to be applied to Dr. Rosenberger's numbers. Lieutenant Stratford's are to be considered as approximate, though a very close approximation.

Does Mr. Taylor see any thing like a law or *mean motion* here? Would any prudent man venture upon a prognostic respecting mean motion, from differences of $-90^{\circ}.6 + 1^{\circ}.3$?

There is no doubt that if the apsides do move in one direction, there has been a time when the path of Halley's comet would nearly intersect the path of the earth; and I find roughly, this was when the elongation of the perihelion from the descending node was about $80^{\circ} 50'$; but *when* was this the case? Such a question, I suspect, does not at present admit of an answer, with respect to a "revolving body solicited by various external and internal attractions in quick succession," and which, moreover, has so very eccentric an orbit,—a matter of more importance in these researches than Mr. Taylor supposes.

Quitting these loose speculations, which offer no grasp to a sound intellect, I will examine Mr. Taylor's application of his hypothesis, viz. to account for the diminution of splendour of the comet "since the time of its recorded appearance 2200 years ago, at the birth of Alexander the Great, and afterwards at the birth of Mithridates." As he quotes no authority, I can only refer to the best, viz. Pingré, in his *Cométographie*, vol. i. p. 264.

Lub. Ricc., &c. En 356 et 336, c'est-à-dire au commencement de la vie et du règne d'Alexandre, il parut deux Comètes; elles durèrent l'une et l'autre soixante-dix jours, au rapport de Justin.

On this statement, drawn, I presume, from Lubienietski and Riccioli, Pingré remarks:—"J'ai lu tout ce que Justin dit d'Alexandre; il dit que sa naissance fut précédée et accompagnée de plusieurs prodiges: IL NE PARLE EN PARTICULIER D'AUCUNE COMÈTE. Mais des Cométographes Astrologues ont pensé que le commencement du règne d'un conquérant

movements have the same cause—the action of the other planets, and by the periodical configurations of those planets both movements are subject to occasional modifications.’

“But I must conclude, lest you, or some one else, should address both parties in the words of Gay’s Cloddipole:—

“‘Forbear, contending louts, give o’er your strains,
An oaken staff each merits for his pains.
But see, the sun-beams bright to labour warn,
And gild the thatch of Goodman Hodge’s barn.
Your herds for want of water stand adry,
They’re weary of your songs—and so am I’

“*Liverpool, Sept. 29, 1845.*

“JOHN TAYLOR.”

tel qu’Alexandre, avoient dû nécessairement être annoncés par l’apparition de quelque Comète éclatante et de longue durée.” Mr. Taylor’s fact is quite worthy of Mr. Taylor’s explanation. As THERE WAS NO COMET, he has accounted *satisfactorily* for its EXTRAORDINARY SPLENDOR, by the MOTION OF THE APSIDES WHICH IS UNKNOWN.

Nor is he much more fortunate with respect to the comet at the birth of Mithridates. Pingré says, p. 270, under the year 136 B.C., on the authority apparently of Justin:—“A la naissance de Mithridate, une Comète parut et dura soixante-dix jours: le Ciel paroissoit tout en feu; la Comète en occupoit la quatrième partie, et son éclat étoit supérieur à celui du Soleil; elle employoit quatre heures à se lever, autant à se coucher.” But there is not an iota of evidence to connect this with Halley’s comet, nor is the date of the birth of Mithridates known. His death certainly happened in the year 62 B.C.; but authors differ so much in his age, that he may have been born in 137, in 134, or 131 B.C. I hope Mr. Taylor, when he next launches into the field of cometary speculation, and makes “public his opinions on physics and astronomy,” will not forget Mrs. Glasse’s rule for cooking a hare, “*First catch it.*” As his phenomena with his explanations (*definitions*, I presume, would scarcely be a synonymous word here, whatever “N. Bailey’s very good dictionary” may say)

“Are melted into air—into thin air,”

it is but charitable to give him another toy to play with. Let him assume the elements of Halley’s comet, and having twitched the line of apsides and nodes according to his pleasure, let him ascertain whether this “hazy star” can have been visible for seventy days together in or about Asia Minor. His *cometarium*, especially if he does not scorn my simplification, may suffice for the problem, and when identity is found to be possible, he may venture on a bolder flight. Let him ascertain the *whereabouts* of Halley’s comet at the time of Alexander’s birth, and its perihelion passage nearest to the presumed birth of Mithridates. This might seem difficult to a man of ordinary endowments, but Mr. Taylor may perhaps succeed with his *peculiar* method of parabolic sectors, aided by the celestial perennial globe and Laplace’s general expression of the most advantageous factors; especially if he first extricates the mathematical theorems concealed by the ancient mythologists under the cup of Tantalus, the stone of Sisyphus, and the wheel of Ixion. I may refer the reader to the memoirs of M. Edward Biot and M. Laugier, already cited, as an antidote to the dreams of Mr. John Taylor of Liverpool.

COMETARIUM AND PERENNIAL GLOBE.

Since the commencement of my controversy with Mr. Taylor a few astronomical notices have appeared from his pen in the columns of the *Liverpool Mercury*. There is little to be said of the cometary portion: they are either extracts, rather spoiled in the process of transferring, or belonging to the class of *incredibilia*. But there are two items of greater importance which Mr. Taylor has thought worthy of a separate form and of being published abroad, as a specimen, I presume, of his acquirements.

“A NEW COMETARIUM.

“Description of an Instrument for finding the Geocentric Positions of a Comet, of which the parabolic or elliptic elements have previously been determined,¹ invented in the year 1834, by John Taylor, and published in the *Raccolta Scientifica* for August 15, 1845, a work intended to give the results of the most recent and esteemed theories and experiments in physics and mathematics, and published twice each month in Rome, by Dr. Clemente Palomba, Associate Astronomer at the Observatory of the Gregorian University, in the Collegio Romano, of the Company of Jesus.²

“Let a circle (No. 1) be laid off, two inches in diameter, and divided into 360 degrees, assigning thirty degrees to each of the signs of the ecliptic (Aries, Taurus, &c.), the centre of the circle will then be the position of the sun, and let a diagram for the comet, conformable to the supposed elements of its orbit, parabolic or elliptic, be constructed on the same scale as the primary circle (No. 1), the sectors for the comet's true anomaly being defined for

¹ Mr. Taylor has not described his Cometarium correctly. Not only must the parabolic or elliptic *elements* be known, but the actual orbit and the comet's places must also be previously laid down to scale, *for which no directions are given*.

² If the *Raccolta Scientifica* contains memoirs like this, it may be intended to give the “results of the most recent and esteemed theories,” &c., but it is centuries behind its date. Dr. Clemente Palomba must have been misled by an amiable anxiety to shew favour to a stranger and an Englishman, or Mr. Taylor's memoir may, like Father De Vico's, have *gained* in being translated. The *new cometarium*, as it is rather facetiously called, consists of an apparatus of circles to perform a very simple operation, viz. having laid down the places of the earth and comet on cards, which represent their orbits, inclined at the proper angle, to find the place of the comet in the heavens as seen from the earth. It will be best understood by astronomers when they are told that it is the common problem of converting heliocentric into geocentric latitude and longitude, with the exception of two short steps, viz. to get the heliocentric longitude and latitude from the data already mentioned.

each five, ten, or more days, before and after the perihelion, as may be convenient.

“The inclination of the comet’s orbit to the ecliptic, the ascending node, and the longitude of the perihelion being known, the sectorial line, that is, the radius vector of the comet’s orbit, which will coincide with the line of the nodes, will also be known. Make these two lines coincide, placing the focus of the parabola or ellipsis on the centre of the described ecliptic circle (No. 1), and the course of the comet, as far as it may be visible, will be made apparent, by giving to the plane of the comet’s orbit its proper inclination to that of the ecliptic.

“For the working of the instrument there must be two sets of these diagrams, the one for the northern and the other for the southern hemisphere; the order of the signs on the ecliptic (Aries, Taurus, &c.) being reversed for the diagram of the southern hemisphere.

“A heliocentric projection of the orbit of the comet, and its position relative to the orbit of the earth will thus have been obtained. Then, let a moveable circular plate (No. 2) be fitted into the common wooden horizon of a celestial or terrestrial globe, the brass meridian being left standing as a semicircular arc for the measurement of altitude; and let this moveable circular plate (No. 2) be divided from its centre into sectors of five degrees each, and let its circumference be also divided into 360 degrees, to represent the visible horizon from the point occupied by the centre of the earth, reversing the marking of the signs (Aries, Taurus, &c.) on this moveable plate (No. 2) for its use in the problems for the southern hemisphere. Let a circular plate (No. 3), corresponding in size with No. 1, the primary circle for the ecliptic, and of the same thickness, be entirely cut out of the moveable horizontal plate (No. 2), so that the circumference of the circle (No. 3), so cut out, shall precisely touch the centre of the large moveable plate (No. 2).

“Again, let a circle (No. 4), two inches larger in diameter than the first assumed ecliptic circle (No. 1) be described round the centre of the large horizontal plate (No. 2); let the plate No. 2 be cut through according to the last described circle (No. 4), and then let an under plate (No. 5) be glued to the large horizontal plate (No. 2) of a size corresponding to it, leaving No. 4 loose, and let No. 5 be supported by a rim or by brackets, so that these two moveable horizontal plates (No. 2 and No. 4) may be made to move correctly from the same centre within the wooden horizon, and both have their surfaces on a level with the external wooden horizon. The plate No. 4 must be made to revolve horizontally round an axis fixed in the under plate (No. 5), the upper surface of that axis being made flush with that of the plates No. 2 and No. 4, and having a small perforation drilled through it, so that a fine line from that central point may represent the line of vision from the point supposed to be at any time occupied by the centre of the earth.

“These preparations having been made, the geocentric longitude

and geocentric latitude of the comet for any particular time may be ascertained; for by finding the earth's place in the ecliptic marked on the circle No. 1, and bringing that point to the common centre of the plates No. 2 and 4, then moving horizontally the circular plate No. 4 till the line which joins the common centre of the circles No. 2 and No. 4 with the centre of the primary circle No. 1 shall coincide with the line which joins the common centre of the circles No. 2 and No. 4 with the place of the sun at the given time, as marked on the circumference of the large plate No. 2, and farther bringing the point of the comet's place, as marked on its parabolic or elliptic orbit for the specified time, to touch the line stretched from the common centre of the circular plates No. 2 and No. 4, to the graduated vertical brass meridian, the geocentric longitude of the comet will then be seen marked on the circumference of the horizontal circular plate No. 2, where it is intersected by the vertical graduated brass meridian; and the geocentric latitude, northern and southern, as it may be, will be shewn by the point where the stretched line cuts the same graduated vertical brass meridian; for the comet then is made to occupy its proper place in the sphere visible from the point occupied by the centre of the earth, and the stretched line is the line of geocentric vision extended into space.

“The diameter of the earth's orbit having been adapted for the scale, the geocentric and heliocentric distances of the comet may be known by the extension of a pair of compasses from point to point.”

“JOHN TAYLOR.”

“*Liverpool*, 4th June, 1845.”

This account of the new cometarium was published in the *Liverpool Mercury* of Sept. 5, 1845.

³ The difficulty which the reader will find in comprehending this long list of circles, double diagrams, &c., arises from their uselessness. Mr. Taylor has got himself into a maze with the multiplicity of his contrivances and the inaccuracy of his language. His circle No. 2, from the use he makes of it, must represent the ecliptic, and not “the visible horizon from the point occupied by the centre of the earth,” which words have the air of being arrant nonsense. “A heliocentric projection of the orbit” is also an incorrect expression. I see no need of any circle after No. 2. A fine line passing from the centre of No. 2 to the brazen meridian would enable any one to bring the earth and comet into the plane of the meridian; the geocentric latitude may then be read off on the meridian, and the difference of longitude between the sun and comet on Circle 2 if the *radii vectores* of the earth are continued backwards to the circumference of No. 1.

Mr. Taylor seems to fancy that an angle when measured by a brass meridian is better measured, and that a diagram somehow attached to a globe mounting is more intelligible than without these unnecessary appendages. Let me give him another and simpler *cometarium*, which requires nothing but a protractor, or scale of chords, and a common card. I will take up the problem after the diagram No. 1 is formed, with only this alteration, that the orbit of the earth is to be drawn upon a stiffish card, and not cut out afterwards.

Mr. Taylor's invention, or at least the communication to the world of the produce of his inventive powers, has been very considerable this year. A globe mounted after his fashion, and by him called a *Perennial Celestial Globe*, has lately appeared, and with it a laudatory notice from the author in the *Liverpool Mercury*, which has been reprinted and circulated as a meet companion to his cometarium. Mr. Taylor shall, as it is fit, speak for himself:—

All that is wanted is to find the point in which a perpendicular from the comet meets the ecliptic, for any contemporaneous places of the earth and comet. When this point is found, and the line joining it with the sun is drawn; the geocentric longitude of the comet is equal to the longitude of the sun (to be taken from the *Nautical Almanac*) + or — the angle subtended at the earth by the line just drawn (to be measured by the protractor); and the geocentric latitude is the acute angle at the base of a right-angled triangle, of which the hypotenuse is the distance between the earth and comet (to be measured by the compasses), and the base of which is the distance from the point aforesaid to the earth, commonly called the *curtate distance*. The difficulty, therefore, consists in finding where the perpendicular from the comet meets the ecliptic.

Take a common card, double it nicely across the direction of its length, so that the two halves may exactly coincide. The card, when left free, will then be like a quarto book half open, and the edge will be perpendicular to the planes which pass through the top or bottom. Place this simple tool, like an upright book half open, on the plane of the ecliptic, and make the edge touch the place of the comet. The point at which the edge rests on the plane of the ecliptic is the point required, and is to be marked with a pin very nicely. This solution of the problem is more accurate than Mr. Taylor's, and requires nothing but a pair of compasses and a protractor, which must have been employed in making the diagram. I trust no one will think that I propose this *cometarium* as a thing of real use, and still less that it should be published in the *Raccolta Scientifica* as a "*result of recent and esteemed theory*."

Mr. Taylor's scale is that of one inch to the earth's radius, the angles are to be measured in a very inaccurate manner, and it is not easy, practically, to place two cards, representing the orbits, very nicely at a given angle. I think it improbable that Mr. Taylor with his cometarium would come to within 5° of the comet's place, and he might be out twice as much, so that it is not even sufficient for *finding* a faint comet. If drawn on a larger scale, it would be difficult to maintain the plane of the comet's orbit. It is only a "hazy" philosopher of the Laputa school who would propose such a toy for a graver purpose than that of boarding-school illustration—just to shew what was the general relation between the paths of the comet and the earth, and something like its general course.

In another place, and at another time, it is intended to give plain directions for obtaining the apparent place of a comet from the elements. With a good scale and compasses it is easy to come ten times nearer than by Mr. Taylor's roundabout, lumbering circles, and brazen meridian.

" ASTRONOMY.

" 'Let my faint lamp at midnight hour
Be seen in some high lonely tower,
Where I may oft outwatch the Bear,
With THREE-GREAT HERMES, or unspear
The spirit of PLATO, to unfold
What Worlds, or what vast regions hold
Th' immortal Mind, that hath forsook
Her mansion in this fleshly nook.'—MILTON.

" A NEW CELESTIAL GLOBE.

" *Description of the Perennial Celestial Globe, invented by JOHN TAYLOR, of Liverpool, and published by Messrs. Malby and Co., of London, Manufacturers and Publishers of the Globes of the Society for the Diffusion of Useful Knowledge.*¹

" The object of the Perennial Globe is to exhibit the change of place in the solstitial and equinoxial points, thereby giving the diurnal revolution of the heavens, as well as the course of the sun and planets for any year, past or to come, in the same way as the globes hitherto

¹ The principle of the perennial globe is, that the celestial globe is supported on a spindle which passes through the poles of the ecliptic; that this spindle is held by two collars in a divided circle, which, therefore, represents a colure; that the colure is itself suspended by poles at a distance of $23\frac{1}{2}^{\circ}$ from the poles of the ecliptic in a second-divided circle, which thus may be made to represent any meridian for any epoch. Mr. Taylor has extracted so much from Professor De Morgan's work, that he might as well have added the sentence at the beginning of the paragraph, which gives a far clearer account of the advantages of his globe than he has done. "The *perennial globe* is a contrivance in which there is a motion of the stars about the pole of the ecliptic, and also one about the pole of the equator, so that all the diurnal phenomena can be represented for any epoch, however distant from our own, past or future:" to which the following note is appended: "This (the *perennial globe*) is the contrivance of John Taylor, Esq., of Liverpool. Senex, an old globe-maker of notoriety, used to make globes on a similar principle, but different in their details. These are described in Benjamin Martin's work on the *Use of the Globes*, chapter ix., and by Costard, in page 132 of his *History of Astronomy*. As far as I can judge from these descriptions, which are not very intelligible, Mr. Taylor's plan is better than that of Senex."

The *invention*, therefore, of the distinctive property of the Perennial Globe does not belong to Mr. Taylor at all, although, by omitting the sentence just quoted and its accompanying note, he appears willing to take the credit of it. It is, at least, as old as Senex, and has probably been *invented*, if such a word can be applied to so simple a contrivance, scores of times before Senex and since. A friend tells me he made such a globe twenty years ago. I agree with Professor De Morgan that Mr. Taylor's globe is better than that of Senex. Instead of a complete circle, Senex employed two arms of $23\frac{1}{2}^{\circ}$, which must necessarily have been less firm and workmanlike; but a reference to Hipparchus's astrolabe will shew that the arrangement of Mr. Taylor's circles is 2000 years old.

in use give those of the present time. The equipment of this globe makes any globe that may have been properly constructed for a given date *perennial* in its action; and thus saves much time in the calculation of the longitude, right ascension, and declination of the stars for different periods; of the places of the perigee and apogee of the sun; of the number of days between the solstices and equinoxes of any year; and of the sun's place on any required day; serving to regulate the calendar for remote periods, which has always been found a matter of great difficulty.²

"This globe also will be found useful, and, in fact, necessary, to those who are desirous of an easy mode of acquiring a distinct knowledge of the science of astronomy, verifying the records of astronomical facts, and illustrating the allusions of the classical authors to the celestial occurrences of their times. The following details of this invention are chiefly extracted from the treatise on the globes recently published by Professor De Morgan, of University College, London."³

Then follows a description of the perennial globe, taken, with some omissions and additions, from a work entitled *The*

² In a very rough way, the perennial globe will probably do a good deal of what Mr. Taylor describes, i. e. if you don't mind being out a degree or so, and if you give considerable latitude to his expressions. The modern globes do not give the courses of the planets, which are taken from the Ephemeris. The "saving much time in the calculation of the longitude, right ascension, and declination of the stars for different periods," is all nonsense, if more is meant than that it gives an *approximation* to the changes of right ascension and declination which are caused by precession. The obliquity is assumed to be constant, so that if the globe were otherwise perfect and convenient it would be of very limited use in examining the old catalogues. A person who can add and subtract would prefer, I think, a short table on a card to Mr. Taylor's contrivances for the places of the perigee and apogee of the sun, &c. What is meant by "regulating the calendar for remote periods, which has always been found a matter of great difficulty," I do not comprehend. All the use of the perennial globe in this way is limited to expressing coarsely what has been calculated nicely in some other way. At least, there is no description of any new problem, shewing how to regulate the calendar by its means, in Professor De Morgan's work or in Mr. Taylor's additions.

³ If the reader will turn to Costard, pp. 132, 150, he will find the problems which were to be solved by Senex's globe; and the perennial globe will do as much, and probably rather better. There are several allusions in the classical authors which may be explained by it, and it may be a useful companion to any scholar, especially while reading Aratus, Hesiod, Manilius, Ovid, &c.; but to assert that the knowledge of the *science* of astronomy will be facilitated by the perennial globe is an exaggeration. When a person has mastered the problems of the ordinary globe he will find little difficulty in understanding the phenomena caused by the rotation of the pole of the equator round the pole of the ecliptic.

Globes, Celestial and Terrestrial, by A. De Morgan, London, 1845, pp. 122, 126, to which I refer the reader:—

“Finally, there is that in the Perennial Globe which no globe on the common construction can supply; and it is, in fact, the only effectual improvement in celestial globes which has been made since they were first invented in times beyond the record of history.”⁴

“JOHN TAYLOR.”

“*Liverpool*, 15th September, 1845.

Printed in the *Liverpool Mercury*, September 19, 1845.

SIR JAMES SOUTH, HIS PENSION, AND ADVERTISEMENT.

At page 80 a note will be found in which I have spoken of Sir James South and the 300*l.* a-year which Sir Robert Peel, being badly advised, bestowed on him to aid his astronomical researches.

As soon as the pamphlet was worked off, copies were sent to the editor of the *Liverpool Mercury* and to Mr. John Taylor, and received by them in the spirit with which they were sent. Previous to publication, I posted a few copies to my friends, and desired one to be sent to Sir James South, as a *party concerned*. A direction from my own hand might have been construed into a personal affront or a personal compliment, neither of which it was my intention to give.

After opening the parcel and discovering the name of the

⁴ The *improvement* is not due to Mr. John Taylor, so I may more freely express an opinion that, for ordinary purposes, it is no improvement at all. The neglect into which Senex' globe on the same construction has fallen proves this, and the need for such an instrument is not now greater than in olden times. The perennial globe is more rickety than the ordinary globe; there is a large space near the pole of the ecliptic blotted out by Mr. Taylor's devices; the meridian and horizon are so far off the globe that the reading is troublesome or inaccurate. If I wanted to investigate *carefully* any of the points for which it is best adapted, I should myself prefer another arrangement. I hope the demand for the globe will repay the spirited publisher; but if purchasers expect that it will justify Mr. Taylor's encomiums, they will undoubtedly be disappointed. The problems which will be found in Costard's book at and after p. 132 are well worth being referred to by those interested in these matters, and who are not able to interrogate the globe without such help.

author, Sir James applied to my printer for information, who told him, as I understand, exactly what I have just said; that, as the pamphlet contained some allusions to him, it was thought fair to give him early and direct notice of the fact. Sir James's answer, which was inserted as an advertisement in the *London Times*, is as follows:—

(*Times of October 1, 1845.*)

"ADVERTISEMENT.

"*The Rev. Richard Sheepshanks and the Liverpool Observatory.*

"*To the Editor of the Times.*

"SIR,—I very much regret having again to soil my pen in writing the name of the Rev. Richard Sheepshanks.¹ As, however, aware from past experience, that by me, as well as by other gentlemen whom I might name, no communication whatever, known to come from him, would be suffered to be left at the house,² his reverence has hit upon the cunning contrivance of sending me anonymously, and addressed in a handwriting not his own, a pamphlet purporting to be a cor-

¹ Sir James has clearly had Fanny Squeer's eloquent epistle in his mind. "When your newew, that you recommended for a teacher, had done this to my pa, and jumped upon his body with his feet; and also langwedged which I will not pollewt my pen with describing," &c. What need had he to dirty his goosequill? I caused the pamphlet to be sent to him because he was mentioned in it, and he, after knowing the cause and returning the pamphlet, as he says, unread, scribbles an advertisement in the *Times*, which is no reply at all. Next to peace, I like honourable, and open, and chivalrous warfare,—a fair notice, a fair challenge, and a fair field. Sir James better likes the practice of "hitting and getting away," which is a very unknightly proceeding. Once before, when I explained the object of his pension in a daily print, I informed him that I was the author, and I shall send him this Appendix, as I before sent the pamphlet, because his ignorance of the way gentlemen should behave must not make me forget what is due from all honourable men, though I dare not hope that he will follow the example.

² Breaking now into the *corpus delicti*, Sir James contrives to write sentences of such involution that I must separate the charges before I answer them.

First, he styles me his "reverence," which rare device is the staple of his letter. Reasoners like Sir James cannot shape an argument out of any material without cutting their fingers. Is "reverend" a term of reproach in Sir James South's vocabulary? Most assuredly not, he would reply, *only* when applied to a certain black sheep, viz. myself. Yet Sir James blunderingly applies to me, as a term of abuse, a name common to every member of a profession which he affects, or did affect, to hold in great respect. The original *gamin* of the Borough is too strong for the would-be courtier.

Some years ago Sir James South attacked me in the *Times* under the vague designation of "ecclesiastical malevolence," which was about as definite as if I were to raise my voice against him under the Euphuism of

respondence between Mr. Taylor and his reverend self on the subject of the Liverpool Observatory ; and which my servants, not knowing whence it came, accepted this morning at the postman's hands, as a matter of course. I think it right thus publicly to inform the reverend divine that on the printer of his work assuring me that he himself sent it me by his reverence's orders, I returned it into the printer's hands without having read one word of it ; that I mean to continue in all the ignorance I now enjoy of its contents, unless some friend should inform me that his reverence has so meddled with me in his pamphlet that my perusal of it is indispensable,³ satisfied as I am per-

"pharmaceutical ignorance." He was forced to unravel his own mystery in a subsequent number, and justified himself by writing to the bishops individually, informing their lordships that his "swashing blow" was meant for me, and not, as many persons believed, for their lordships. I warned him then that he had better let our respective professions alone. The valuable class of medical practitioners, to which he belongs by birth and education, has been too often made the butt of saucy wits to leave the dullest man without the power of retort. Were I like-minded with my adversary, I could "knog his gallipots about his knave's costard," (speaking *metaphorically*, for I have no intention to give occasion for a *criminal information*), but I will not be provoked into such a contest. I pray you "reform it altogether." My profession has no connexion with the very simple question,—what has Sir James been doing these fifteen years to "justify his patron's bounty?" The answer, if answer there be, may be given in half the space of the advertisement, and surely we have waited long enough for it.

Secondly, Sir J. South says—or rather means to say, for his English is scarcely better than his Greek—that he and "some gentlemen he might name" had declined any communication with me, and that I was aware of the fact. Some years ago a gentleman, then very intimate with Sir James, and acting, as I suppose, in concert with him, did secure the last word in a discussion which he had provoked by returning a letter to me unopened. This, I thought then, and think now, was a piece of bad manners. Though I have had some opportunity of repaying the intended affront with interest since, I have not yielded to the temptation, perhaps because I did not feel the point of the insult. Sir James copied this example on the following occasion.

While under cross-examination respecting his large equatoreal, he declared, with more valour than discretion, that "he was as competent to perform certain astronomical computations as I" was, and *proposed* "that we should be shut up in a room for trial." I immediately *accepted* the challenge, and sent him a note soon after to remind him of it, which he returned unopened, guessing, I presume, the contents. Now it is shrewdly suspected by those who best know him, that Sir James, in geometry, has never crossed the *Asses Bridge*; that in algebra, he took fright at the abstruse properties of + and —; and that in computation, he never employs logarithms or trigonometrical tables, and for the *best possible reason*. If this be so, and I am pretty sure of the fact, there was some prudence in eschewing investigation ; but the mode is scarcely to be commended. Except Sir Andrew Aguecheek, I never heard of any man who thought it a good jest "to challenge one to the field, and then break promise with him, and make a fool of him." Sir James has odd notions of humour.

³ I do not know whether Sir James has any friend who will inform him of the nature and extent of "my reverence's meddling" with him ; but if he had fifty such friends the result would be the same. The mention of some

fectly with the following sentiments of the original correspondence, by a disinterested person—a man of education and a gentleman—who, as editor of the *Liverpool Mercury*, writes in that journal of the 23d of May last as follows:—

“THE REV. R. SHEEPHANKS. — This personification of the *cacoethes scribendi* may be assured, that had we been able to find sufficient space for his first moderate letter, it would have appeared at once; but the fact is, that far more important communications than his, which were in type before it was even written, have not yet gained

friend is just a tub for the whale, merely thrown out to cheat his readers into a belief that he *could* say something if he *would*. Sir James South, when he wrote his advertisement, had no more idea of replying to me than he had of substantiating his thirty-nine libellous charges against the Royal Society, or of being shut up in a room with me for examination. It is one of his many doubles, and one to which he has frequently had recourse,—a sort of promissory-note for some distant and unnamed day which will scarcely prove negotiable. His notion of looking after his character by deputy is a shift too, but more original. When Sir James next reads his *Æsop's* fables he may perhaps learn that a man had better mind his own interests himself.

“This is a more humiliating acknowledgment of incompetency than I ever expected from the once confident and blustering Sir James South. He is so closely in communication with Mr. Taylor, the *Liverpool Mercury*, and its editor that he carefully keeps by him a scrap of impertinence, which is untrue both in fact and opinion. I am almost certain that he has read Mr. Taylor's half of the controversy; and if so, he knows that, in the main point—the necessity for a meridian mark—I am right and Mr. Taylor wrong. Sir James in his working days, *i. e.* some *twenty years ago*, had no meridian mark, and I alluded to him at p. 31 for that reason. He dare not make himself responsible for Mr. Taylor's follies, and he is neither so prudent as to give no opinion, nor so honest as to give a true one. He does not see that by espousing a side *confessedly without examination* he places himself in a dishonest as well as in a foolish position. That Sir James should condescend to pick up his opinions in practical astronomy from the editor of a newspaper, himself a sort of disciple to Mr. John Taylor, of Liverpool, is “blackening his own face and eating much dirt” with a vengeance.

But the editor of the *Liverpool Mercury* is a disinterested person—a man of education and a gentleman.

“Make your vaunting true,
I shall be glad to learn of noble men.”

The editor will have some difficulty, I fancy, in proving his *disinterestedness*, and in his proper person only undertakes to be funny. His education I don't question, except that it does not include mathematics, or astronomy, or logic, or Italian, or any thing touched upon in my dispute with Mr. Taylor; and as to his being a gentleman, he shall have the appellation from me when he shews by his conduct that he deserves it. Sir James's testimony on such matters reminds me of what Gibbon says respecting a certain Abu Rafe, the evidence to a miraculous tale of Ali's prowess. “Abu Rafe was an eye-witness; but *who will be witness for Abu Rafe?*” Sir James has pledged himself to so many things which have never come to pass, that his credit is rather *below* par. Should he feel inclined, or should “some friend” think it “indispensable” for him, to dispute this assertion, let him call upon me for my proofs.

a niche in our columns. We now rejoice at our escape from any connexion with his subsequent productions, for of all the specimens of arrogant assumption and Billingsgate language we ever met with, his writings present the fiercest concentration. They are utterly unbecoming a gentleman or a clergyman, and prove the author to be even less acquainted with the virtues of good temper and humility, which ought to adorn a Christian minister, than he confesses himself to be with the mathematics.'

" Your obedient Servant,

" J. SOUTH."

" *Observatory, Kensington, Tuesday night, Sept. 30, 1845.*"

A REPLY

TO

MR. BABBAGE'S LETTER TO "THE TIMES,"

"ON THE PLANET NEPTUNE AND THE ROYAL
ASTRONOMICAL SOCIETY'S MEDAL."

BY THE

REV. R. SHEEPSHANKS, V.P.R.A.S., &c.

Brut. O name him not, let us not break with him ;
For he will never follow anything
That other men begin.
Cass. Then leave him out.
Casca. Indeed, he is not fit. *Julius Caesar.*

LONDON :

PRINTED BY GEORGE BARCLAY, CASTLE STREET,
LEICESTER SQUARE.

1847.

By an oversight, the whole of the sheet was composed in the same type : to distinguish between the text and commentary, it was found easier to alter the character of Mr. Babbage's portion, as being the smaller in quantity.

A REPLY

TO

MR. BABBAGE'S LETTER IN "THE TIMES."

IN *The Times* newspaper of March 15, a letter from Mr. Babbage was inserted, with the heading, *On the Planet Neptune and the Royal Astronomical Society's Medal*. Some remarks on this letter were made by the Astronomer Royal, and by the Editor, in the *Athenæum* of March 20. Before meddling in the matter, I thought it right to give Mr. Babbage time for a rejoinder. But as nothing has appeared from him, and as the Council allowed the last meeting to pass over without paying any attention to Mr. Babbage's complaint, I venture to assume the task of confuting Mr. Babbage's letter, paragraph by paragraph. A few words of introduction will save some trouble to my readers.

On the 13th of November last, the Astronomer Royal read before our Society a historical notice of such facts concerning the new planet as had come particularly to his knowledge. This memoir was printed in our *Monthly Notices* for November, and has since been reprinted by Professor Schumacher, in the *Astronomische Nachrichten*. At pages 129, 130, is a letter from Mr. Adams, stating the mass and elements of an unknown and exterior planet, which, according to his calculations, would account for the observed irregularities of *Uranus*; and also a table of the remaining errors of *Uranus*, after the corrections due to the exterior planet had been applied. Mr. Adams' letter was received by the Astronomer Royal in the last days of October, and was answered by him a few days after, on November 5, 1845. These results were also communicated to Professor Challis (pp. 129 and 145). It is admitted that the orbit was defined, and the place of the planet predicted by Mr. Adams, with sufficient correctness in this letter; and, so far as I have heard, no one has called in question the accuracy of any part of the statement made by the Astronomer Royal.* If Bremiker's map had been in existence, and employed in searching for the planet at the time of Mr.

* I am reminded of a silly article in the *Mechanics' Magazine*, but I presume this to have been a hoax on the editor, and not a deliberate calumny, without a particle of proof or probability to support it.

Adams' letter, the planet would have been found almost, if not altogether, as easily in November 1845, on Mr. Adams' prediction, as it was found, owing to that map, by Dr. Galle, in September 1846, on M. Le Verrier's prediction.

Mr. Adams did not communicate his *investigation* till November 13, 1846, when he placed it in my hands as Secretary of the Society. It has since been printed and profusely distributed, thanks to the activity of Lieutenant Stratford, and the liberality of the Lords of the Admiralty. As the numerical results of the first portion of this investigation are identical with those of the letter of October 1845, the first part of Mr. Adams' investigation must also be prior to that date, as he himself affirms.

The second part of the investigation would seem, and for the same reasons, to be anterior to Mr. Adams' letter of September 2, 1846 (pp. 137, 8, 9), dated two days later than M. Le Verrier's second paper on the same subject, which was presented to the French Academy on August 31, but not printed till some time later.

If the proof that Mr. Adams had detected the existence and place of a planet exterior to *Uranus*, as early as October 1845, be considered sufficient, and if the discovery of the new planet be understood to be the detection of its place, mass, and orbit, by solving the inverse problem of perturbation, and finding the planet by its effects on *Uranus*, then I think it would be difficult to deny Mr. Adams the title of *first* discoverer.

At the meeting of Council on December 11, the names of all the gentlemen to whom the medal might by possibility be awarded, were proposed and seconded, in accordance with the bye-laws; M. Le Verrier and Mr. Adams being both proposed, and both by the Astronomer Royal.

On January 8, the day prescribed by the bye-laws, the Council met at an early hour to award the medal, and before any formal motion was made in favour of any candidate, one of the members read a statement of some length, analysing, as well as he was able, the claims of Mr. Adams, and concluding by a suggestion that the usual medal should be given to M. Le Verrier, and that the Council should apply to the General Meeting next month for power to grant another medal to Mr. Adams. This suggestion was not favourably received by a majority of the members, and to bring the matter to an issue, a motion was made and carried that no application should be made to the General Meeting: hence only one medal could be given. On this the Astronomer Royal, who had before expressed his opinion strongly on the necessity of giving two medals, declared that he would vote against one medal,

whoever the candidate might be, as he thought it would mark a greater difference than was just: the result of the ballot was that a majority of three to one, which is required by the bye-law, was not obtained for M. Le Verrier or for any candidate.*

So far as I understood the disposition of the members of the Council, it was this. All, or almost all, were anxious to give a medal to M. Le Verrier. A large minority was anxious to give a medal to Mr. Adams also. The majority considered a medal to Mr. Adams an injustice to M. Le Verrier, while a minority of one-third considered a single medal to M. Le Verrier an injustice to Mr. Adams. And I firmly believe that every member gave his vote to the best of his understanding, the only obligation by which members of Council are bound.

At the Annual General Meeting on February 12, Mr. Babbage, after a short speech, first proposed, if I remember, something or other, which he withdrew on being told that it would be met as a vote of censure. He then proposed the following motion:—

“That this meeting express their deep regret that the Council have not awarded the Society’s medal to M. Le Verrier, for his publication of the greatest astronomical discovery of modern times;”

which was negatived by a very large majority.

There are, I think, these objections to Mr. Babbage’s motion, on the very face of it: that it pointed out no remedy, and that the merit to be rewarded is said to be the *publication* of the discovery, and not the discovery itself. A publisher is sometimes not the author of the thing published, and this may happen by fraud or by consent. Though I do not, for instance, dispute Mr. Babbage’s invention of a calculating machine, of some sort or other, and to some extent, the *publication* of the invention, so far as it has been published, has, for the most part, been anonymous or extraneous, and, therefore, the merit, according to the wording of Mr. Babbage’s motion, not his.

What is the proper conclusion to be come to in the following cases?

In Mr. Babbage’s work *On the Decline of Science*, the remarks on practical astronomy and instruments (I mean all those which are not obvious or erroneous), we know to

* There is great impropriety in publishing private and confidential conversations, but so much was told in the Annual Report, and so much was said at the General Meeting, that I see no use in *any* reserve, especially as the spirit of the Council discussion has been mistaken and misrepresented.

be contributions. Are they Mr. Babbage's own, because he published them? It is notorious that all the descriptions of instruments, most of the calculations, and the few sound philosophical remarks contained in Sir James South's memoirs, are borrowed, though not acknowledged lights. Did he, by publication, make them his? If Mr. Babbage says Yes, then let the daw be set down for a genuine peacock, since there is no doubt he *published* himself in peacock's attire.

I must request the Fellows to correct a press error in Lieut. Raper's amendment. The words, "and awarding a medal to M. Le Verrier," must be inserted after Section 16 of the "bye-laws."

Mr. Babbage's Letter to "The Times" of March 15.

“ON THE PLANET NEPTUNE AND THE ROYAL ASTRONOMICAL SOCIETY'S MEDAL.

“Mr. Babbage presents his compliments to the Editor of *The Times*, and hopes that the great interest taken by the public in the history of the new planet will admit of the Editor's giving publicity to the accompanying letter, which, although placed on Friday last in the hands of the Chairman of the Extraordinary General Meeting of the Astronomical Society, for the express purpose, was not read at the meeting.”

The Chairman may explain *why* he did not read Mr. Babbage's letter to the meeting. I shall content myself with shewing that it *ought* not to have been read, and *could* not have been read in regular course. The relation of the letter and its contents to the "History of the New Planet," I leave to the writer of the letter. Mr. Babbage should have designated the "meeting of Friday last" by its peculiar title, viz. *Special* General Meeting. The word *special* has a more definite meaning than extraordinary, and is the word used in the bye-laws.

“A few words will explain the previous circumstances. It is in the power of the Council of the Royal Astronomical Society, to confer one medal annually upon the most important astronomical discovery of the year. But to prevent the award of medals to unimportant discoveries, a bye-law requires that there shall be a majority of votes of three to one in order to confer a medal.”

There is a whole section in our bye-laws by which the medal is created, and the mode of proceeding directed. Mr. Babbage's language implies that there is only one bye-law, and that its object is merely to fix the necessary majority.

There is no restriction as to time. The medal may be conferred, so far as I know, several years after the appear-

ance of the work for which it is awarded. Neither is it limited to *discoveries*, in any intelligible or usual sense of the word. Any work of sufficient astronomical merit is considered to bring the author within the law.

Mr. Babbage has stated, as the sole motive for establishing a majority of three to one, that which is not hinted at in the laws themselves, and which certainly is only one reason among many and stronger reasons. The lawgivers thought a majority of three to one, on the whole, the best, and that is all we know about the matter. Try Mr. Babbage's mode of reasoning in one of our best known institutions, the trial by jury. The law requires unanimity. If Mr. Babbage were one of four jurymen, who felt clear on one side, would he recommend his friends to yield their opinion up to the eight others on no better ground than that the case was "most important," and that "their privilege was conferred upon them by society," only to be exercised when "unimportant" matter came before them?

"At the Annual General Meeting of the Astronomical Society on the 12th of February last, the Report of the Council was presented to the Society. Although in other respects highly interesting, it announced the fact that they had not awarded the medal to M. Le Verrier, and assigned reasons for the omission entirely unsatisfactory to many of the members present. At the subsequent discussion and adjourned discussion, which occupied that and the following day, it was admitted that the proposition made in the Council for giving the Society's medal to M. Le Verrier was supported by ten votes, while the Astronomer Royal with four other votes opposed it. Having therefore only a majority of two to one in its favour, the proposition was negatived."

The Report of the Council, with the statement I have already made, will put the case before the Fellows more fully than Mr. Babbage has done. The Council could not award a medal because there was not a legal majority for any candidate. The reasons assigned in the Report are principally reasons for not suspending the existing bye-laws, which certainly do not appear to me of any weight. As the medal rests with the Council alone, the propriety of interfering with their decision may be doubted. The Society can alter its bye-laws, and choose its council, but there is a manifest inconsistency in keeping the bye-laws and re-electing the same members of Council, while disabling their judgment in a matter which is declared to be within their peculiar province. In awarding a medal the Council sit as judges, and ought not, and do not regard any suggestion whatever but that of their own consciences.

In the discussion which took place at the General Meeting, the Astronomer Royal and some other members of the

Council mentioned how they had voted, and why, which is not conveyed by Mr. Babbage's impersonal phrase, "it was admitted."

"Four other amendments were successively proposed by Mr. Raper, by the Rev. R. Sheepshanks, by Dr. Whewell, and by the Astronomer Royal. The last only was adopted. In consequence of this an extraordinary General Meeting of the Society was summoned for the 12th of March, for the purpose of authorising the Council to award two or more medals, and to communicate that award at the ordinary meeting on the 9th April."

Mr. Babbage has left his own part of Hamlet out of his own playbill. *He himself* moved the first amendment on the resolution, "that the Report be received and adopted." There were three, not four, other amendments proposed, for Dr. Whewell assuredly proposed none.*

"It was at this extraordinary general meeting that the following letter ought to have been read, in which Mr. Babbage hoped at once to satisfy the claims of justice, and to reconcile the conflicting opinions of opposite parties, by proposing to enable the new Council to give the ordinary medal, distinguished by the date of 1846, to M. Le Verrier, whilst a special medal, bearing date 1847, should be created for the express purpose of rewarding the equally eminent merits of Mr. Adams."

I shall give some reasons hereafter, why Mr. Babbage's letter ought not to have been read; and I wonder how he could expect any effect whatever from his letter, if it had been read a score of times. The main object of his proposition,—to enable the Council to give two medals,—was the very proposal of the Astronomer Royal, which we were met to discuss, viz.—"That the Council be authorised to award two (or more) medals, if they shall deem it expedient to do so." Mr. Babbage's additional conditions, specifications, and explanations, could have had no effect but that of throwing ridicule on the proposal, and causing its rejection. The "usual" medal could be no more granted to M. Le Verrier, than yesterday can be recalled. The dates appended by him to the medals are one or both erroneous, whether they apply to the time of discovery or to the award; and, what Mr. Babbage seems never to have contemplated, the new Council would certainly have followed its own notions of propriety, without regard to his recommendations. As to Mr. Babbage's "hope of satisfying the claims of justice and reconciling the conflicting opinions of opposite parties,"

* Dr. Whewell spoke (I believe to my amendment), and alluded to a better mode of proceeding. He referred, probably, to the amendment subsequently proposed by the Astronomer Royal, which had been written out and shewn to him, and which he held while speaking.

I should just as soon expect a covey of Mother Cary's chickens to foretell halcyon days, as that Mr. Babbage should be a successful promoter of harmony.

"It is only necessary to add, that the meeting came to the resolution, 'that there was no reason for reconsidering the matter.'"

This is not quite correct; for, after a good deal had been said, the meeting agreed to the following amendment, moved by Mr. De Morgan, upon the motion of the Astronomer Royal:—

"That on taking all the circumstances into consideration, and particularly the existing differences of opinion on the subject, it is not expedient to propose to the Council to reconsider the subject of the medal."

Mr. Babbage's mistake arose from quoting the brief summary of a friend, for the very words of the amendment.

"MR. BABBAGE'S LETTER TO THE CHAIRMAN OF THE EXTRAORDINARY GENERAL MEETING OF THE ROYAL ASTRONOMICAL SOCIETY, HELD MARCH 12, 1847.

"SIR,—As the present Extraordinary General Meeting of the Astronomical Society has in some measure arisen from an amendment moved by me at the late Annual General Meeting, I think it due to the Society to apologise for my unavoidable absence."

Mr. Babbage's amendment had the same relation to the Special General Meeting, as Mrs. Hayes had to the king—

"The king himself has followed her
When she has walked before."

He took up a good deal of time and helped to spoil a pleasant dinner, and that was all he effected, good or evil.

"I have carefully examined all accessible papers relative to the new planet, and am very reluctantly compelled at the last moment, by my medical adviser, to forego the intention of explaining personally my views to the General Meeting.

"As I take a warm interest in the success and character of the Astronomical Society, I am anxious to be permitted very shortly to state the course I should have advocated."

The cause of Mr. Babbage's absence is to be regretted; but it may reasonably be doubted whether his presence would have contributed to the harmony or intelligence of the meeting. In the previous discussion, which lasted two days, Mr. Babbage contributed nothing from his accumulated store of information but his spurious "modern law of discovery."

1. "The modern law relating to discoveries is, that they take date from the time of their first publication to the world."

Mr. Babbage has stated this so positively, that most peo-

ple would fancy he had some authority for it. Yet no one else has heard of the law; it is not supported by analogy, or usage, or common sense. It is, I believe, an *ex post facto* law, enacted by the sole authority of Mr. Babbage himself, to suit a particular case. Waring laid down a rule, declaring "that person to be the first discoverer who first published his discovery, or who, at least, first communicated it to his friends;" and though the rule is not considered to be satisfactory, it has hitherto been pretty generally acquiesced in.

The confusion about discovery, and first discovery, may perhaps be somewhat cleared up by a little attention and a more careful use of words.

To constitute a discovery, something new must be so specifically declared that there is no doubt as to the matter discovered. To prove the date of the discovery, it is generally necessary to shew that it has been communicated, and the date of the communication is taken for the date of the discovery.* The two points for consideration are,—1st. Do the facts adduced contain clear evidence of a discovery?—2ndly, Is the communication itself, and the date of the communication, quite certain?

But it would be evidently unjust towards other persons if the first discoverer should assume a right to *exclude* every one else from the territory to which he himself has *publicly* shewn no title. Hence, I should hold that a second discoverer has equal, or *quasi*-equal rights with the first, if the first discoverer has not made his claim publicly known.

The word *publication*, including every kind of communication, from a private letter up to a Royal Proclamation or Act of Parliament, has been the cause of much ambiguity.

If it were laid down for law, that a claim to discovery and the date of that claim must be proved strictly like other matters of fact, and that the right to exclude all succeeding discoverers can only date from the *evulgation*† of the discovery, and no earlier, probably the justice of the case would be as well provided for as it need be. The relative merits of the different parties must be settled after a careful examination; but the *evulgator* ought to have a preference to the *first discoverer* in cases of doubt. It is owing to the *laches* of the first discoverer that any difficulty arises, and it is fair enough that he, if any body, should suffer for the trouble he has unnecessarily caused. But it would be unjust and absurd, as well

* It is easy to conceive cases of exception to the rule requiring communication. The date of a discovery is a matter of evidence, the *onus probandi* resting on the claimant.

† The word is purposely coined for the occasion. It is intended to express publication by printing, by open lectures, by transmission to learned societies, and the like.

as cruel, to deny a positive fact from motives of convenience, and to say a man is no discoverer at all because he is a modest or a careless one: it is quite sufficient punishment to give him a partner in that which he might have had all to himself.

"In this case I think there can be no doubt as to priority of publication. Mr. Adams, guided by the same spirit which led him to his brilliant and successful inquiry, has himself fully admitted it.

" 'I mention,' says Mr. Adams, 'these dates merely to shew, that my results were arrived at independently and previously to the publication of those of M. Le Verrier, and not with the intention of interfering with his just claims to the honours of the discovery: for there is no doubt that his researches were first published to the world, and led to the discovery of the planet by Dr. Galle, so that the facts stated above cannot detract in the slightest degree from the credit due to M. Le Verrier.' *On the Irregularities of Uranus, caused by a more distant Planet.* By J. C. Adams, Esq. Appendix to the *Nautical Almanac* for 1851, p. 5."

Except the unquestioned fact that M. Le Verrier was the first to make his results public by the press, this extract proves nothing. Mr. Adams admits the *just* claims of M. Le Verrier, and so does every one else; and he also asserts his own. He gives no opinion as to *relative* merit, and if he did, Mr. Adams is just the person whose opinion ought not to be of any value. I consider Mr. Adams' candour to be on a par with his extraordinary mathematical talents and acquirements; but the moral and intellectual qualities have no *identity*, as Mr. Babbage seems to suppose. A man may be a very considerable analyst without a particle of candour, and with very little common sense in common matters.

2. "I concur entirely with the majority of the late Council, that the last medal ought to have been awarded to M. Le Verrier. And I much regret that the small minority of that Council should have availed themselves of a privilege conferred upon them by the Society, to prevent the awarding of medals to any discovery not eminently deserving, into the means of preventing any such award in the strongest case which has yet occurred during the existence of the Astronomical Society."

Mr. Babbage's intention "of reconciling the conflicting claims of opposite parties" must surely have deserted him when he penned these offensive and impertinent sentences. Luckily Mr. Babbage has formed an erroneous estimate of all parties, as well as of his fitness to be a judge and mediator between them. It is curious enough, when the whole of Mr. Babbage's communication to *The Times* is taken into consideration, that his censure will be found to fall rather on the majority of the Council, whom he

lauds, than on the minority, whom he blames; and it is still more odd that, of all the members, I should be the one whose conduct he must most approve. Mr. Babbage allows that "the claims of justice would be best satisfied" by granting the usual medal to M. Le Verrier, which was my suggestion to the Council in January last. Moreover, Mr. Babbage thinks a second medal necessary, "to reward the equally eminent merits of Mr. Adams," which was also suggested by me, and which also was the opinion expressed by the Astronomer Royal at the Council. We, then, the minority in the preliminary vote, have the full approbation of Mr. Babbage. When a simple majority had decided in favour of one medal only, I felt myself bound to vote for M. Le Verrier, as undoubtedly deserving the medal. So that Mr. Babbage must entirely concur with me, and only partially with the majority who refused to notice Mr. Adams' claims at all, or with the minority who voted against M. Le Verrier, rather than abandon them. I beg, however, to point out some specific differences between Mr. Babbage and myself, notwithstanding this apparent agreement. My proposal was made at the proper time to the proper persons, and before any discussion or irritation had taken place. Mr. Babbage seems to have stumbled on his idea two months later, after exhausting other combinations, and when the time was gone by; besides this, he clogged the simple proposition with irrelevant and absurd details, dictating unnecessarily and offensively to the new Council how they were to distinguish the medals.

I have already pointed out Mr. Babbage's error in asserting that one among many reasons was the *sole* reason for the majority demanded by the bye-law. If he will take the trouble of reading the section relating to the medal, he will find that the law allows a vote by ballot and prescribes a majority of three to one. The vote by ballot is *always* resorted to in practice, without any special demand, and it may be demanded as a right by any member. Now a vote by ballot implies that no one is to know how his neighbour votes, and it is only by eluding the ballot* that any one can certainly know whether he is in a majority or minority. It is clearly intended that each member is to vote according to his own conviction, and the ballot-box is introduced for the very purpose of securing perfect freedom. I am no advocate of secret voting (except where bribery or intimidation is to be feared), but I cannot see that any one is

* For instance, by taking a preliminary show of hands, which has been done to procure unanimity, but which is plainly against the intent of the ballot.

to be blamed for voting according to his judgment; and the law expressly relieves him from all responsibility, except that to his own conscience.

"I had intended to have proposed that the usual medal (call it that of 1846) which, in the opinion of a large majority of the last Council and of a large number of our members, ought to have been awarded to M. Le Verrier, should be placed at the disposal of the present Council; and that an extraordinary medal should be created for the present year (call it that of 1847), and that it also should be placed at the disposal of the Council."

I suppose the idle clause at the head of the proposal is in furtherance of the design "of reconciling the conflicting opinions of opposite parties." It was the less calculated to produce this effect, as the whole of the culpable minority are, I believe, members of the new Council. The usual medal could not be granted to M. Le Verrier, for the time had gone by.

The medal of 1846 was presented in February 1846 to the Astronomer Royal. Mr. Babbage meant, I presume, to grant the lapsed medal of 1847 to M. Le Verrier, and to date it accordingly; and also to take the future medal, that of 1848, out of the disposal of the new Council, and to give it, during the current year, with the date 1848, to Mr. Adams. If I may take the liberty, I should say Mr. Babbage's first date was an oversight, the latter a blunder. I cannot fathom Mr. Babbage's notions of propriety, nor do I see the fitness of giving the later-dated medal to the earlier discovery, and so to "reward the equally eminent merits of Mr. Adams."

Suppose the offensive passage to be withdrawn and the absurd dates erased, Mr. Babbage's proposal was already before the Meeting.

I hope it is now evident that the Special General Meeting of March the 12th lost very little wisdom, and just as little good humour, by the omission of Mr. Babbage's letter of instructions. Except his peculiar recipe for restoring harmony and "reconciling conflicting opinions," the contents of the letter had been heard from Mr. Babbage's own lips a month before, without attracting any attention.

Neither has he any just cause to complain of a want of respect. It will be seen from Section X. of the bye-laws that, when a Special General Meeting is called, "a week's notice must be sent to every Fellow residing in or near London of the business for which it is summoned, and no business shall be brought forward at such meeting, except what has been so notified." The "business notified" was the motion of the Astronomer Royal, and it was the resolution of several

members to enforce the bye-law strictly. The consideration of Mr. Babbage's *additions* to "the business notified" would have been specifically objected to as illegal, in whatever way their introduction might have been attempted.

After the mover and seconder had spoken, Mr. De Morgan moved an amendment, which was carried. It is impossible to say that Mr. Babbage could not have introduced his propositions while speaking on the amendment, but it would have been a nice task. If they had been proposed in a distinct form as matters to be debated, there can be no doubt, I think, that the attempt would have been stopped. Mr. Babbage, therefore, could not have advocated the course he wished to recommend.

If Mr. Babbage's propositions could not have been formally laid before the meeting by Mr. Babbage himself, it is clear that he could not do that by letter which he could not have done personally; and even if the form and substance of the letter had been in order, I hold that it ought not to have been read from the chair: the proper way would have been for some confidant to introduce it as part of his own speech. It is easy to shew this by shewing the absurdities which would have arisen if Mr. Babbage's letter had been read, as he intended, by the Chairman to the meeting. The imputation on the minority of the Council would have been undoubtedly resented, and its omission demanded. Who would have had the responsibility of defending or erasing the offensive passage? Mr. Babbage's dates are wholly incomprehensible: who was to explain their meaning, or alter them, or omit them? Certainly not the Chairman, or Mr. Babbage's official blame-bearer, the Secretary.

I do not think that a letter from an absent member could be read with propriety. The meeting was a strictly legal meeting, guided by the usage of the House of Commons—so far as it is understood. Can absent members write letters to the Speaker with amendments on motions about to be debated, and insist on their letters being read from the chair? To make the privilege of any use, the letter must be read first, and thus a question might be debated by written speeches before the original motion was even opened. In any case, if any one be to blame it is the Secretary, and not the Chairman, for it is the province of the Secretary "to read the letters and papers presented to the Society."

But if Mr. Babbage has no ground for complaint against the Society, or against any officer of the Society, for not reading his letter, they have great reason to complain of him for thus recklessly accusing them to the readers of *The Times*. These form, no doubt, a very numerous, and,

speaking generally, a very intelligent body, but not one which can or will give much attention to such uninteresting disputes, if the Editor could and would allow the assailed party full space for reply. But this he certainly would not do, and it is obviously impossible, for a slashing accuser can bring as many charges in a sentence as can be satisfactorily refuted in a column; and who cares enough for truth, simply as truth, to read answers which have no other merit? I acquit Mr. Babbage of a deliberate wish to mislead, but the obvious tendency of his letter is, first, to represent himself as an amiable and slighted man, anxious to "reconcile conflicting opinions," and able to effect this good end, if his nostrum had not been refused; secondly, to convey to his readers a notion that he had peculiar and extensive knowledge on the matters in debate; and lastly, to impute blame to gentlemen whose offence is, that they acted according to law and to the best of their abilities. I think I have proved that these insinuations

"are such stuff

As dreams are made of;"

and if so, Mr. Babbage is in a very unsatisfactory position with respect to "the Royal Astronomical Society and their officers."

May I call Mr. Babbage's attention to another medal, granted by the Royal Astronomical Society many years ago for the "valuable invention of an engine for Calculating and Printing Mathematical and Astronomical Tables?" This award was made so long ago as June 13, 1823, and we are now in April 1847,—a good way off the publication of this valuable invention, and at a very indefinite distance indeed from the chance of reaping any benefit from it.

I have always understood that the Government advanced very considerable sums to Mr. Babbage for the construction of this piece of ingenious mechanism; but I have never heard any *satisfactory* account of the expenditure, of the delay which has occurred, or of the abandonment of the original design. I say *satisfactory*, because in these matters I conceive the public have a right to something more authentic than hearsays, or anonymous statements, or notes or lectures by secondary parties. Where is there to be found a full, true, and particular account, by Mr. Babbage himself, and authenticated by his name, of the celebrated calculating machine, which was rewarded with a gold medal by the Astronomical Society, and towards which the Government did, as a trustee for the public, advance a considerable number of thousands of pounds?

I do not pretend to feel any very intense interest on the

subject, never believing, or having any cause to believe, that the so-much-vaunted machine would be of any practical use in calculating mathematical and astronomical tables,—with many better judges, I have considered this machine to be a misdirection of Mr. Babbage's acknowledged talents for analytical speculation and research, and a misapplication of public money: but it is desirable to have the whole matter cleared up on many accounts.

I have such confidence in Mr. Babbage's integrity in money matters, that I am sure every farthing advanced by the country has been expended on the machine; but this is not known to all the world, and there is some truth in the remark, that one honest man who *will* give no account of his trust, does as much injury to society as ten rogues who *can* give none. This is the true ground for blaming the great Scipio, or, in a less way, I suppose, Mr. Alderman Gibbs: they afford the

“Exemplar vitiis imitabile,”

which is a defence to men of less purity.

Allowing that Mr. Babbage may have reasons plenty as blackberries for all that he has done and omitted to do in the matter of the calculating machine, he may be assured that a full and connected statement of particulars by himself would be a great relief to some of his friends, and to many who are not connected with him. There is a good deal of dissatisfaction and a great want of information on the subject; and a recollection of the calculating machine has often stood as a scarecrow in the way when applications for government assistance have been contemplated. How can any one comfortably suggest,—to the most liberal minister, and in the most prosperous state of our finances,—a grant in aid of any laborious scientific research, when he is conscious that the allowance of 300*l.* a-year to assist Sir James South in his astronomical observations has not produced a result worth recording in sixteen years; and when, so far as is generally known, Mr. Babbage's calculating machine, after a lapse of twenty-three years and an outlay of many thousand pounds, has been abandoned as a total loss,—nothing produceable but iron bars and brass wheels?

LONDON:

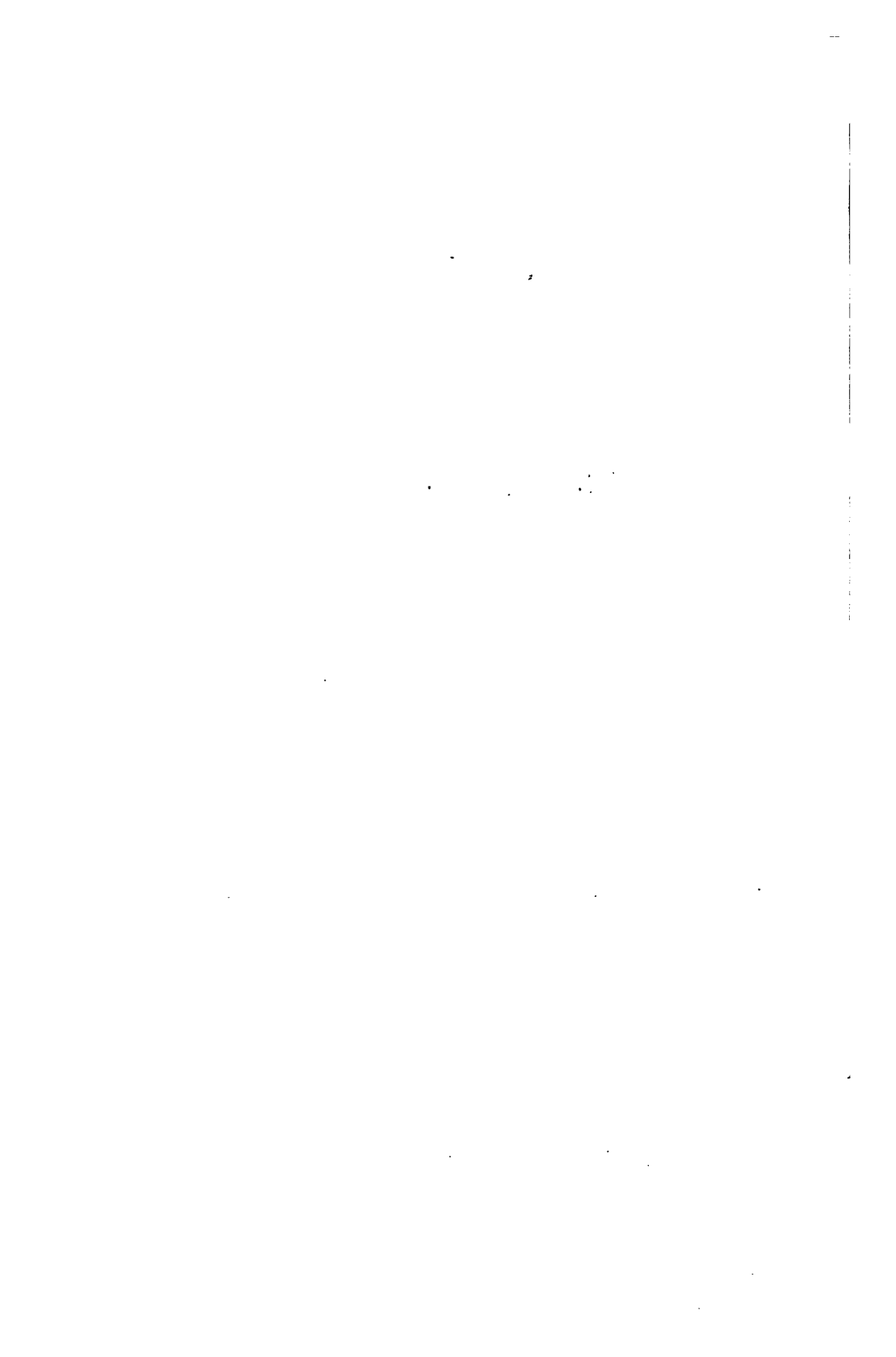
GEORGE BARCLAY, CASTLE STREET, LEICESTER SQUARE.

ON THE
CONSTRUCTION
OF
STANDARD THERMOMETERS.

BY THE
REV. R. SHEEPSHANKS, M.A. F.R.A.S., &c.

EXTRACTED FROM THE MONTHLY NOTICES OF THE ROYAL ASTRONOMICAL SOCIETY.

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*On the Construction of Standard Thermometers.**

On considering the different stages of a complete and satisfactory solution of the problem which has engaged me so long, viz. how to fix the standard yard, and to multiply trustworthy copies of it, I was very early led to inquire, as a necessary part of my task, into the reliance which could be placed on such thermometers as were in ordinary use or easily procurable. My very constant friend and coadjutor, Mr. Simms, gave me every information in his power; but very frankly admitted that he did not profess to make thermometers possessing the degree of accuracy I seemed to require, and that, to the best of his knowledge, there were no thermometers to be had which would at all come up to my mark. After a little inquiry and some consideration, in which I received much valuable information from Professor Miller, I came to the conclusion that if I wanted to have thermometers on which I could thoroughly rely, I must make them, that is, *graduate* them, myself.

I now proceed to explain the method which I have adopted to produce a standard thermometer; and though the trouble is great, and some doubts perhaps hang still upon a part of the process, I believe I can produce, with tolerable certainty, instruments which, with certain precautions, will show a thirtieth or even a fiftieth of a degree of Fahrenheit. I only require that skill in the workman which is usually to be found, and I do not *depend* upon him at all.

The two forms of thermometers which I graduate are those which have tubes divided into 180 or 120 equal parts; the divisions are bit with fluoric acid on the glass. The divisions which I use are a tenth of an inch apart, and they are drawn on the wax covering the tube by Mr. Simms' straight-line dividing engine. It is desirable, though not strictly necessary, that the divisions should be equal, and it is also desirable that the bore of the tube should be pretty uniform. The maker will, "for a consideration," select the tubes; and it is worth while to pay the additional price, as it saves a great deal of trouble. I do not know whether the circumstance was accidental, but I found some round bores almost perfectly true: this has never been the case with the flat bores. The latter, however, are much more easily read. The flat bore must have no sensible *twist*.

The bulbs should not be very small or very large: I find from three or four tenths to half an inch or a little more in diameter is a convenient size. In my longer tubes, those of 18 inches, I direct the maker to have the freezing-point as near the zero of division as he can safely get it, only it must be *above*.† The boiling-point must also be within the graduations, and the nearer 180° the better.

* The substance of this account was delivered orally at the June meeting of the Royal Astronomical Society, when the apparatus was exhibited.

† The tubes may be divided before the bulb is blown, but this is not necessary.

I call these, *generating* thermometers; for, as will be seen hereafter, no thermometer is constant in *wide* ranges and at *different* times. The divisions in a well-contrived thermometer of this kind are, of course, rather greater than degrees of Fahrenheit, but not much.

The thermometers which are divided into 120 equal parts may have the freezing-point either high or low, according to their proposed use. For ordinary use, in this climate at least, I should wish to have the freezing-point somewhere about 10 divisions, and to have the 120 divisions equivalent to about 100° of Fahrenheit. I have reason to believe that, even at this temperature, the bulb of the thermometer suffers a small change, which continues for some time, though not permanently. For nice observations at usual temperatures, the freezing-point might be brought very near the zero, and the upper end of the scale made to represent 90° , or even 85° Fahrenheit.* It is better to have special instruments for special purposes, and, if extreme cold is to be measured, to have one instrument for this service.

The first and indeed only essential manipulation to learn, is that of *breaking the column* of mercury. On holding the bulb uppermost, the mercury will run down to the farther end of the tube, and a *vacuum bubble* will show itself on the bulb. If the tube be now quickly and adroitly reversed, the vacuum bubble may be driven into the *neck* of the bulb; and if that is done, a slight tap when the thermometer is horizontal will break off the column of mercury *at the neck* of the bulb. With a little practice this trick is easily learned, and by warming or cooling the bulb, the length of the column to be broken off can be adjusted very nicely to what the experimenter requires. By repeated trials, I can always break off a column to a quarter of a division, or even closer; but this accuracy is more than sufficient, if the tube is pretty regular.

The column should *unite* again, by holding the tube inclined and the bulb downwards; or the mercury may be driven from the bulb to the other end by a smart stroke on the palm of the left hand, with that end downwards, while holding the thermometer in the right, bulb uppermost. If now the bulb be lowered, keeping the vacuum bubble *out* of the neck, the column should unite.

From some causes which I cannot explain—perhaps from little irregularities at the neck of the tube, foulness or imperfection in the tube itself, or impurity in the mercury—it frequently happens that the mercury will either not *break*, or will not *unite*, as it should do. I should recommend the rejection of a thermometer, *for the purpose of a standard*, where these contradictions are frequent, and where the instrument seems refractory. But I have always been able, in the end, to obtain my purpose, though sometimes at a considerable expense of time and temper.†

* A small chamber is or may be contrived at the top of the tube, to prevent breakage by extraordinary expansion.

† Bessel seems not to have known this art of breaking the column, for which I am indebted to Mr. Frankum. Bessel's plan of separating the column by the flame of a lamp is very unsafe. There is the pressure of an atmosphere on the

Supposing this manipulation acquired, and a tractable thermometer provided, having a division upon the glass stem to 180 or 120 equal parts, I proceed as follows. First, with the tube having 180 divisions I break off a column, which, when the tube is horizontal, reaches from 0° to about 90° ; let us suppose to $89^{\circ}4$. I now bring one end of this column to 180° , and read the other, which suppose $89^{\circ}8$. As the same mercury is used in the two measures, it is clear that the *capacity* of the tube from 0° to $89^{\circ}4 =$ the capacity from $89^{\circ}8$ to 180° , or that the mean of the two readings $89^{\circ}6$ will be the point which bisects the capacity of the tube from 0° to 180° . As $89^{\circ}6$ on the tube in question would be expressed by 90° on a *perfectly equal* tube, the *correction* to $89^{\circ}6$ or to 90° is $+0^{\circ}4$.*

The *length* of the column, *i. e.* the length it would occupy if the tube were uniform, is $0^{\circ}2$ short of half-way, or is $= 89^{\circ}8$.

Before proceeding to a new breaking of the column, I carry one end of the present column to 60° , and read the other somewhere near 150° ; and after that one end to 120° , and read the other near 30° . The use of these will be seen immediately. I now cool the bulb, if necessary, and break off a column which is about equal 60° , and this I carry forward at three steps from 0° to 180° , *i. e.* I make one end fall successively on 0° , 60° , and 180° , and read the other end.

It is clear, that if the error of the division of 60° tends to lengthen or shorten the first of these measures, it will tend to shorten or lengthen the second of these measures, and in like manner the error of the division of 120° will have *contrary* effects on the second and third measures; that is to say, the *sum* of the three measured lengths will not be affected by an error in the divisions 60° and 120° , and therefore the *mean* will give the true length of the column, in parts of a *perfect* tube of the same capacity. Suppose this mean to be $60^{\circ}3$, then the difference of the end of the first reading from $60^{\circ}3$ will be the correction to 60° , and the difference of the third reading from $180^{\circ} - 60^{\circ}3$, or $119^{\circ}7$, will be the correction to 120° . These readings, as well as the former, should be repeated until there is no doubt of their exactness. I now carry the column forwards and backwards from 90° , and read the ends towards 30° and 150° .

softened glass, which, without caution, might strangle the tube. When the column is broken above the neck of the bulb, sometimes a union may be effected by turning the whole body of mercury up and down, backwards and forwards. Sometimes the bulb may be cooled, so that the separation enters into the bulb and becomes obliterated. By heating the bulb and cooling it, the larger mass will seem to lick up the fragment. This last process is of no importance while dividing the thermometer; but as heat above 100° will disturb for a time the freezing-point of an established thermometer, this mode of uniting the column is only to be resorted to in case of necessity, and the thermometer should be laid aside for some time to return to its original state.

* On the supposition that the tube is correct enough to permit the assumption that the error *near* 90° does not sensibly differ from the error *at* 90° . This assumption is made throughout; but if the tube is very unequal, special precautions must be resorted to.

Now the *true* value of 90^d being known, viz. $90^d.4$, and the true length of the column of 60^d being also known, viz. $60^d.3$, it is clear that the *true* value of the other ends of the column, after the column of 60^d has been carried backwards and forwards from 90^d , will be $90^d.4 - 60^d.3$ and $90^d.4 + 60^d.3$, or $30^d.1$ and $150^d.7$; and as the *actual* readings are known, the difference from the *true* values will give the *corrections* of 30^d and 150^d . In like manner, since the *corrections* of 60^d and 120^d are known, and the value of the column of 90^d is also known, the column of 90^d carried back from 120^d and carried forward from 60^d will give the *true* values of 30^d and 150^d , and, when compared with the *actual* readings, will give the *corrections* of those two divisions. Consequently, the *corrections* of the divisions 30^d and 150^d are given by two independent methods; and if these agree well, the *mean* must be correct. If there is any discrepancy, the operations must be repeated *toties quoties*, until a satisfactory conclusion is arrived at, though with moderate care and a steady temperature* no error should occur.

I now break off a column of 50^d and ascertain its *true* length, i. e. what it would measure in a perfect tube of the same capacity, by carrying it thrice from 0 to 150^d and thrice from 30^d to 180^d (the *corrections* of 150^d and of 30^d have already been ascertained). If these two values of the length agree, I have got, just as with the column of 60^d , the errors of 50^d and 100^d , and of 80^d and 130^d . I now carry this column backwards from 60^d , which gives me the error of 10^d ; backwards from 90^d , which gives me that of 40^d ; backwards from 120^d , which gives me that of 70^d ; forwards from 60^d , which gives 110^d ; forwards from 90^d , which gives 140^d ; forwards from 120^d , which gives 170^d . There are now only two of the decades missed, viz. 20^d and 160^d . These are got by carrying 50^d backwards from 70^d and forwards from 110^d , but as these are derived secondarily, it is prudent, in a former stage, to carry the column of 60^d backwards from 80^d and forwards from 100^d . If the two independent values agree nearly, a mean may be taken, and there can be little doubt of the result.

By breaking off a column of 45^d and carrying it four times along the whole distance from 0^d to 180^d (or from the two extremes and from 90^d), its *length* may be obtained. Carrying this backward from 50^d , 60^d , &c. the *corrections* of 5^d , 15^d , &c. may be ascertained, and by carrying it forwards from 0^d , 10^d , 20^d , &c. the *corrections* of 45^d , 55^d , 65^d , &c. are obtained. The *corrections* of the divisions into fives from 45^d to 135^d are thus got twice over, and independently, which is a great confirmation, and the accuracy of the mean is much increased.

When the tube is tolerably good, i. e. when the errors of the divisions follow a sort of law, I have found a careful interpolation of the *corrections* of the tens sufficient, without actually measuring

* The expansion of the column at first frequently plagued me; but since this was allowed for I have had no trouble, except from mere blunders of reading off, or in arithmetic.

If the observer is careful to assure himself that each stage of the operation is correct before proceeding to the next (and the verifications almost force themselves upon him), the work will proceed to the end without any vexatious harking back. The subdivision can be carried below the fives, but this is scarcely ever necessary. It is better to reject a manifestly imperfect tube than to employ so much time as would be necessary to correct each

A = $\frac{1}{2}$ sum of 1st differences above and below the horizontal line.
 — $\frac{1}{12}$ sum of 3d differences — — —
 + $\frac{1}{60}$ sum of 5th differences — — —

B = $\frac{1}{2}$ 2d difference on horizontal line.
 — $\frac{1}{24}$ 4th difference — —

C = $\frac{1}{12}$ sum of 3d differences above and below the horizontal line.
 — $\frac{1}{60}$ sum of 5th differences — — —

for $-x$ the interpolated value $= M - Ax + Bx^2 - Cx^3$
 $+x \quad \quad \quad = M + Ax + Bx^2 + Cx^3$

the interpolation where $x = \frac{1}{2}$, or $\frac{1}{4}$ or 0.1, 0.2, &c., is very rapidly performed, and the checks so satisfactory, that error can scarcely creep in.

single division, though I have done it very satisfactorily in a tube which had a manifest strangulation at one part.

I have described the process as it may be conveniently performed *by eye*, and it is desirable to examine each tube in this way, even when a more careful determination is proposed. I do not think that by mere estimation a tube can be divided with greater accuracy than 0.1 div., though this will depend on the observer. But even this degree of accuracy is beyond what is found in many standard thermometers, and such an examination costs very little time, not, I think, so much as half an hour.

In dividing my own thermometers, I have always read off each end of the column by a short telescope, which is carried by a slide parallel with the tube, the tube itself resting horizontally in two notches. I find the indistinctness of the end of the mercury and the division on the glass is not troublesome, when the distance from the tube to the object-glass is about four times that from the object-glass to the image. If the telescope is not longer than 5 inches, the tube can be touched without disturbing the observer. The micrometer should have a large divided head, and one revolution of the micrometer should correspond nearly to one division on the tube. Thus hundredths of a division are read at once from the micrometer, and thousandths may be estimated.* By moving the telescope nearer to and farther from the object, and by pulling out and pushing in the eye-piece, it would be easy to adjust the micrometer revolution to the division of the tube; but tubes are seldom quite straight, and I find it best to place the fine wire (which is used for observing the end of the column and the preceding and succeeding divisions) upon the division preceding the end of the column by a slow motion in the slide, when the micrometer reads 0; the fine wire is then carried by the micrometer to touch the end of the column, and afterwards to split the succeeding division, both of which readings must be noted. The observations are then repeated and noted, moving the micrometer backwards. Taking for the true reading the mean of the forward and backward readings in each case, the difference of the readings of the preceding division and of the end of the mercury, divided by the difference of the readings of the two divisions, gives the decimal of a division required. A short table of double entry will save trouble in computing when the adjustment is tolerably nice. The remaining computation is the same as in the eye estimation.

An example of the calculation of the errors of every ten divisions in a thermometer graduated to 120 equal parts, will show the operation better than any mere description.

* In my own apparatus, the screw is too fine and the telescope too long, so that a good deal of time is lost in making the observation and in computing the decimals of the division. One division on the tube corresponds to four revolutions of the screw, so that there is a chance of setting down a wrong number of revolutions, a chance which not unfrequently happens.

The readings of the divisions corresponding to the ends of the column, reduced from the micrometrical observations, are as follows :—

Column = $60^d \pm$.

$- 0^d.294$	$60^d.118$	$19^d.628$	$39^d.900$
$60^d.276$	$119^d.706$	$79^d.777$	$99^d.811$

Column of $40^d \pm$.

$0^d.136$	$40^d.147$	$80^d.390$	$19^d.806$	$60^d.212$
$40^d.111$	$79^d.653$	$119^d.600$	$59^d.590$	$99^d.530$

Column of $50^d \pm$.

^d — 0°060	^d 50°218	^d 20°139	^d 70°435	
49°982	99°524	69°852	119°559	
^d 10°209	^d 30°125	^d 40°177	^d 60°299	^d 80°343
60°088	79°682	89°612	109°511	129°374

The — signifies that the end of the column is below the zero.

The operation is nearly the same throughout. The *length* of the column, i.e. that which it would occupy in a *perfect* tube of equal capacity from 0^d to 120^d , is first determined; and this column, having one end near a division which has no correction or a known correction, measures the error and correction of the division near its other end.

Apparent values of column of $60 \pm = 60^d.570$ } By first pair of observations of
 $= 59^d.588$ } $60 \pm$.

Mean or true value of col. = $60^d.079$

Apparent values of column of $40 \pm = 39^d.975$ } By first three observations of
 $= 39^d.506$ } $40 \pm$.
 $= 39^d.210$ }

Mean or true value of col. = $39^d.564$

Correction of 60 Division.

Reading of known end	$- 0^d.294$	$119^d.706$
True length of column	$+ 60^d.079$	$- 60^d.079$
Actual value of other end	$59^d.785$	$59^d.627$ i.e. what each should be.
Corresponding reading	$60^d.276$	$60^d.118$
Correction to 60^d	$- 0^d.491$	$- 0^d.491$ for scale of equal parts.

the boiler begin to chatter, and it is certain that the necessary heat, at least, is attained. By removing one, two, or three of the flat pieces, it will be found that in a little time the position of the mercury becomes steady, and is not affected by closing one of the holes or unclosing another. This steadiness of the boiling-point, whether the steam issues languidly or with considerable vehemence, is rather a puzzle to me, but the fact is quite certain.

A measure of the reading which corresponds to the end of the mercury (by telescope and micrometer as before), and a reading of a standard barometer (with its attached thermometer), the cistern of which is on a level with or at a known height above or below the boiler, give, by a formula, that reading of the boiling-point which corresponds to a pressure of 30 inches, or any other pressure that may be assumed. I propose, however, to determine the relation of the boiling-point at different pressures experimentally.

Two or three cautions must be observed. The boiling must continue a little time, as I think I have observed, to allow the bulb to arrive at its full expansion. The water must not be allowed to get too low, or the flame may heat the boiler *above* the water; and in that case the apparent boiling-point of the mercury will begin to rise.* Lastly,—the caution cannot be too often given,—the line of sight of the telescope must be perpendicular to the tube at the boiling-point, very nearly, or the plague of parallax, no small plague if the tube is thick, will come in.

In contriving my own apparatus I added an appendage, which is certainly satisfactory to a scrupulous observer, though I have scarcely derived any other advantage from it, viz. a steam-gauge in the other end of the boiler, which is thus out of the way of the thermometer and its cork: the gauge is inserted above the water-mark, and so contrived that the steam as it condenses flows back into the boiler. By placing rings to touch the two surfaces of the mercury in the gauge when the water is not boiling, or when all the holes are open, it is easy to satisfy yourself that there is no pressure when the boiling-point is noted, although the steam may issue with considerable violence.

If now, without loss of time, the thermometer thus boiled be placed in ice as before, and the freezing-point ascertained, it will be found to have fallen rather less than $0^{\circ}.3$ Fahrenheit.† The freezing-point rises, however, very rapidly—a very sensible quantity in 24 hours—but I cannot say how long a time it takes to resume its former stand. I am pretty sure that it does return exactly, in time; at least my observations hitherto agree to confirm this

* This happened to me once, and rather alarmed me, till I made out the cause. There are many ways—by balance, for instance—of ascertaining the quantity of water. I have applied a tube, bent upwards, which is inserted below the water-mark, through which I can also feed the boiler; but I have not yet used it.

† This, at least, has been constantly the case with me when the thermometers had been long enough made to have come to a steady stand before determining the freezing-point.

opinion ; though I have heard from an eminent foreign maker that he does not find quite the same thing.

I will here venture to throw out a hypothesis, which I do not remember to have seen suggested elsewhere, and which I propose to examine as soon as I have time ; viz. that the law of expansion is not so simple as has been generally supposed, but consists of two terms. One of these simply depends on the temperature of the body at the time ; the other is a function of the highest temperature to which the body has been elevated, and the time since elapsed. There are several anomalies which I have seen, or heard of, which might, I think, be solved by this law, if it should turn out to be a true one. There is, too, some analogy with a well-known fact, that a piece of metal which has suffered violent constraint, such as hammering, rolling, &c., has a tendency to relieve itself, as it were, in time. I believe that some of the great variations which seem to have taken place in our standards of length may be referred to one or other of these causes ; the lengths have been set out on bars too recently cast, or forged ; or the metal, having been constrained by rolling or drawing, has struggled to release itself after the division.*

To return, however, to the thermometers. Having got contemporaneous freezing and boiling-points, and the errors of the divisions, I find the degree of Fahrenheit which corresponds to each tenth or fifth degree, and interpolate for the rest. After a considerable time I determine the freezing-point anew, which should agree with the *first* value, and add the proper correction to the table, if already formed, or use it in forming my table of equivalents of Fahrenheit.

If I am right in my belief, that the freezing-point of an old thermometer begins to fall even at so moderate a temperature as 100° Fahrenheit, it is clear that a *standard* thermometer, in the common acceptance of the words, i.e. a thermometer which will correctly indicate all temperatures from freezing to boiling, is not to be had, except the thermometer be boiled just before every observation,† when, alone, if I am right, the thermometer is correct at all temperatures below boiling.

For the ordinary usage of a standard thermometer, i.e. marking with great exactness all temperatures from a little below freezing to our greatest summer heats, I prefer using a well-seasoned thermometer divided into 120. The freezing-point is determined as before, and a point between 70° and 80°, or thereabouts, from a comparison with several *generating* thermometers, which have come to a steady freezing-point. By placing the thermometers in a box of water, and comparing with several generators, using the precau-

* The process of annealing might, perhaps, cure this restlessness in the particles of metal ; but time would still be required, if this hypothesis be true, before the mass took up its final and permanent *stand*.

† This would be necessary in measuring the heat of hot springs, if 0°·1 or 0°·2 be sensible in such observations.

tions so often described, it is, I think, possible to get an upper point with sufficient accuracy.* A table, assigning the corresponding values of Fahrenheit, is to be constructed for each thermometer; and these, I should hope, would be truly *standards*, within the limits for which they are designed. A verification of the freezing-points, after a considerable time, would be all that could be wanted to entitle them to perfect confidence. In this way, by the use of two forms of thermometer, it seems to me that all temperatures up to boiling-water may be estimated. It is indeed possible that an extreme degree of cold may alter the freezing-point, but a mixture of salt and snow did not do so; though I own the experiment was carelessly made, not belonging particularly to the subject on which I was and am engaged.

I assume that the thermometer tube is horizontal. In a well-made instrument the mercury will not break at the neck or run to the other end, unless the tube be considerably inclined downwards, and my concern is only with the best tools and the best mode of using them. There is this objection to placing the thermometer upright, that the pressure on the interior of the bulb, when the column is long, enlarges the bulb sensibly and variably, and causes the mercury to stand *lower* than it should do. In thermometers with large thin bulbs, I think that the difference of reading between a horizontal and vertical position is at least half a division. But as in many cases a thermometer *must* stand upright, its errors, when upright, should be determined by comparison with a standard thermometer arranged horizontally. If this were carefully done, at a high and low temperature, the correction due to this cause for the upright thermometer might be applied to the tabular errors or table of equivalents. Perhaps, with care, a thermometer might be compared with itself, in the two positions; by continuing the observations alternately for some time, the effect of small changes of temperature would be eliminated.

I conceive that all *nice* thermometrical observations should be made by telescope and micrometer; the telescope should move on a graduated bar, so as to prevent the possibility of parallax. If this refinement is thought needless, a reading lens may be slipped along a coarsely-graduated rod, when the reading will be free from all uncertainty except that of estimation.

It is obvious that, for most purposes, a thermometer which requires the use of a special table, of a specially-mounted telescope, and of a microscope, will not be required. I conceive, however, that a standard thermometer would always be useful as a check upon commoner instruments. To accompany the standard yard it is absolutely necessary.

* I compare with four, five, or six generators, and take a mean. The generators are perfectly independent, and any small errors in measuring the divisions or fundamental points probably nearly compensate each other. The water should be at the temperature of the room.

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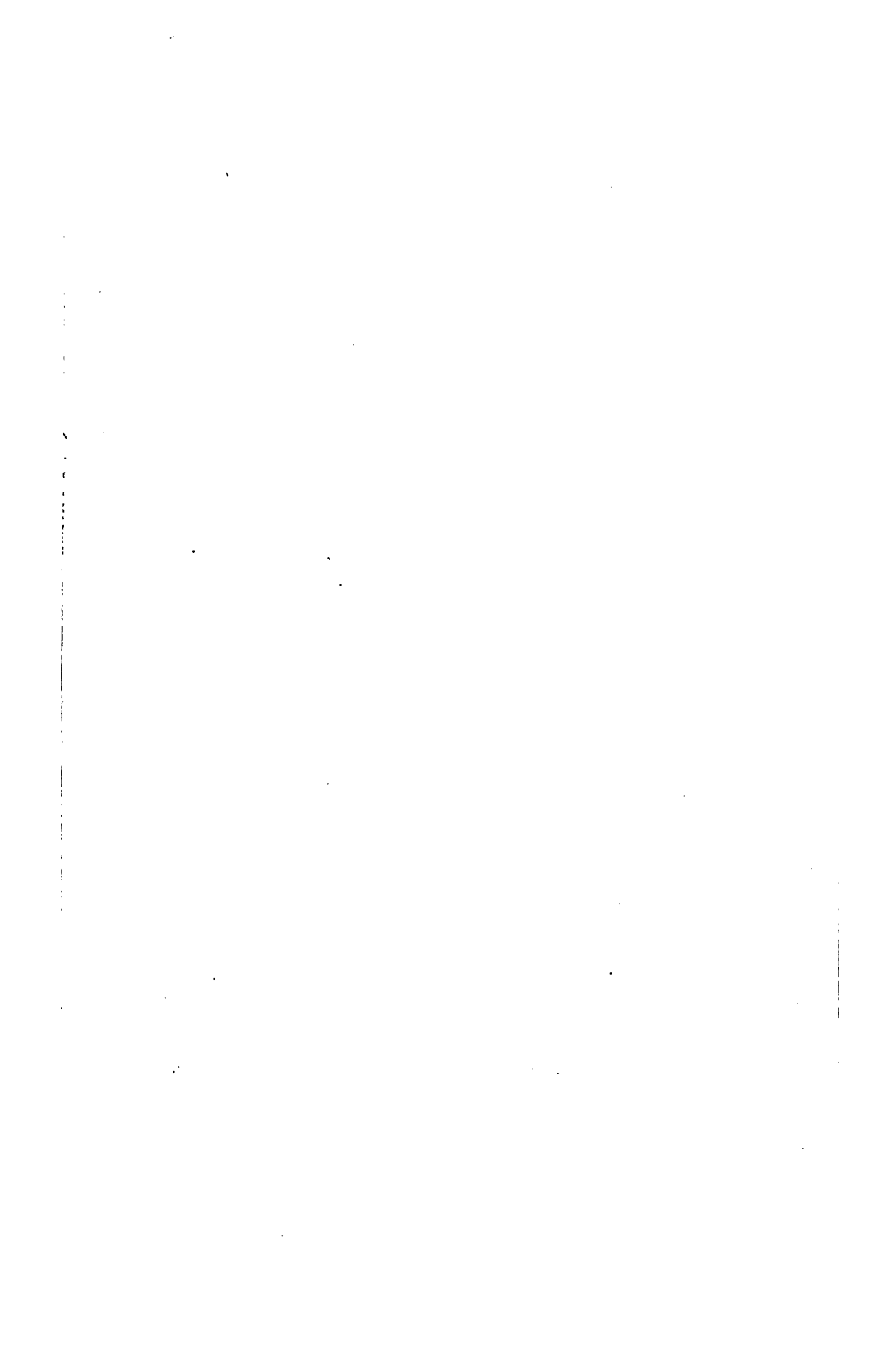
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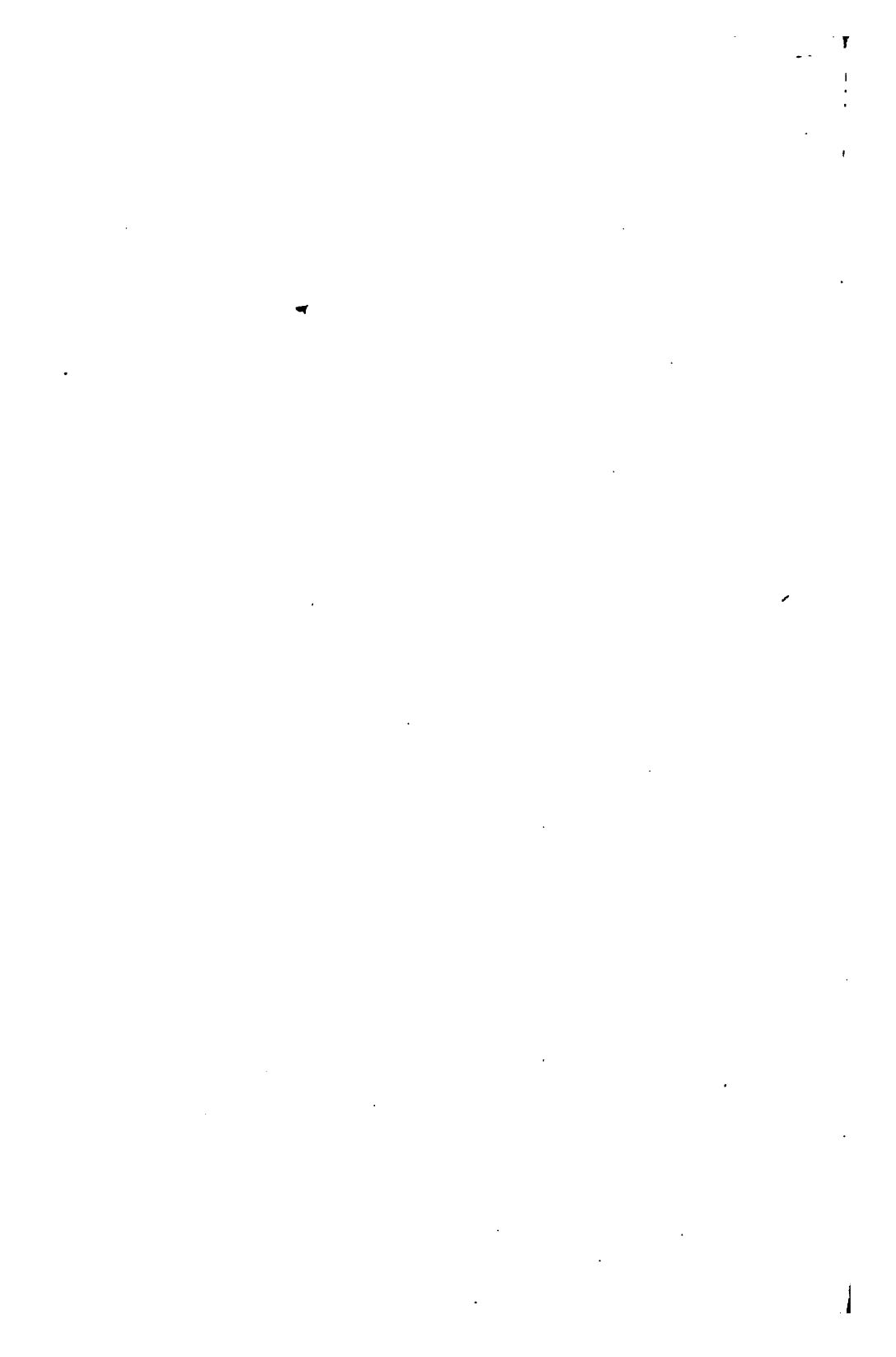
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